Researchers, Ideas, and Economic Growth: Evidence from Vietnam War Draft Avoidance*

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Abstract

Researchers' ideas contribute to economic growth, yet returns to their labor remains unclear. We exploit a unique historical setting to study the effects of researchers' labor supply on knowledge production. During the Vietnam War, draft deferment for graduate education ended in 1967 causing thousands of STEM-trained workers facing draft risk to join the research workforce. Using a new dataset of the scientific workforce during the 1960s in the US, we show that publication output responded strongly to this increased supply of researchers, while patents did not. We link researchers to their firms, thus allowing us to measure productivity effects and the rate of return to R&D. Our estimates imply that in an environment where ideas are getting harder to find, the social rate of return to researcher's efforts remains at least 27%.

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1 Introduction

Claims that shortages in highly educated STEM workers are holding back innovation and competitiveness have played a prominent role in recent legislation, such as the CHIPS and Science Act of 2022.[1] Cross-country data indicate these concerns are not unwarranted. Figure 1-panel A shows that the number of researchers is growing more slowly in leading economies – i.e., France, Germany, UK, and US – than middle-income economies – i.e., China, Korea, Singapore, and Turkey. Figure 1-panel B shows that the growth rate of researchers is correlated with the growth rate of GDP. While researcher human capital is central to modern theories of economic growth since at least Romer (1990), measuring the empirical foundations of its causal effects has proven more elusive.[2]

Three limitations have limited prior progress measuring the return to scientific human capital investment. First, the many datasets that measure the production of ideas – e.g., in terms of academic papers or patents – do not capture the inputs of scientific workers who do not publish or patent. Research effort – the number (and skill) of workers actively conducting research – must be measured to estimate its return. Second, researchers do not invest in knowledge production randomly. Unobserved productivity shocks can lead to both researcher labor supply and knowledge production outcomes. Third, harnessing exogenous researcher-specific shocks to develop a clean research design can lead to micro estimates that capture both spillover as well as aggregate effects. As innovation features both spillover (Azoulay et al. 2010, Moretti 2021) and reallocation (Acemoglu et al. 2018, Atkeson and Burstein 2019) effects, the aggregate and micro effects of researcher supply may be quite

[1] As the White House Office of Science and Technology Policy stated axiomatically in its 2022 report, "Science, technology, engineering, and mathematics (STEM) are the foundation for discovery and innovation." Some policy analysts maintain that Americans' STEM training is farther-reaching. Brookings Senior Fellow Darrell M. West (2023), for example, argues that "more effective STEM education and workforce development are necessary for U.S. firms to have the people they need for domestic production, supply chain management, cybersecurity, data analytics, and innovation in general. The shift to a digital economy requires bold actions by government, business, and higher education. Failure to make necessary changes will slow the transformation and impede the country's ability to benefit from emerging opportunities." The US has a long tradition of supporting STEM education, dating to the Morrill Act of 1862, which established a land grant university in each state "to teach such branches of learning as are related to agriculture and the mechanic arts," though "without excluding other scientific and classical studies." The America COMPETES Act of 2007 and Reauthorization in 2010, as well as the CHIPS and Science Act of 2022, are the most recent examples of significant federal funding to promote the development of STEM human capital.

[2] A large literature following Barro (1991), Mankiw, Romer, and Weil (1992), and Griffith, Redding, and Van Reenen (2004) seeks to relate human capital to economic growth. Recent contributions show that human capital drives regional development (Gennaioli et al. 2013), that human capital quality is critical (Schoellman 2012), and that imperfect substitutability is relevant for human capital's contribution to development (Jones 2014, Caselli and Ciccone 2019, and Rossi 2022).

different.

In this paper we address these challenges using novel data from the National Register of Scientific and Technical Personnel (NRSTP), a census of hundreds of thousands of US scientifically trained workers that was collected during the 1960s. Very importantly, the NRSTP contains information on whether an enumerated worker engaged in research, thus providing a measure of research input available in few other contexts. We link the scientists represented in the NRSTP to disambiguated academic publications and patents data to measure knowledge production. By conducting our analysis based on researchers' "pre-market designations," we obtain estimates of the aggregate effects of researcher labor supply. In our analysis, a researcher's so-called designation is defined at the paired state of birth and undergraduate major level. By conducting our analyses at the level of these broad endowed characteristics based on paired state of birth-college major designations, we capture both the direct and indirect effects of researcher supply. That is, because researchers in our analysis cannot change their permanently pre-defined designations once they have entered the scientific workforce, market reallocation responses that would lead to different micro and aggregate effects are mitigated.

The Vietnam War and the associated military draft was era-defining for young Americans in the 1960s. Between 1964 and 1973, over 2.6 million US military served in Vietnam and over 46,000 US service personnel were killed in the conflict. Among the 8.8 million Americans serving in the military during the Vietnam era, over 1.7 million had been drafted into service, and many no doubt sent to Vietnam (US Bureau of the Census 1977, 369). A coercive draft became necessary because the increased risk of injury or death reduced the supply of volunteers (Altman and Barro 1971). Many sought to avoid being drafted by going to college (Card and Lemeiux 2001), having children (Bailey and Chyn 2020), or even committing crime (Kuziemko 2010).

The dramatic growth in the draft to meet military needs in Vietnam contributed to the public turmoil and tumultuous protests across the nation into the late 1960s. Early in the conflict, those most highly at risk of being drafted – i.e, male US citizens under 26 years old – could defer their draft eligibility by attending college and then graduate school and then perhaps getting married and having children, thus skewing the incidence the draft imposed on different segments of the population. In 1966, President Johnson appointed a commission to "consider the past, present, and prospective functioning of selective service" with foremost respect to "fairness to all citizens," among a number of other considerations.[3] Despite the

[3]Executive Order 11289, "National Advisory Commission on Selective Service," July 2, 1966

president's commission weakly recommending the elimination of all educational deferments, the political negotiations over the reauthorization of the selective service in 1967 led to the elimination of educational deferments *only* for graduate school. We propose that the sudden elimination of a draft deferment for graduate school attendance during the Vietnam War serves as exogenous variation in the supply of STEM-trained human capital in the workforce, generally, and scientific researchers, specifically.[4] Using the NRSTP data, Figure 2 shows that by 1970 the abrupt end of the graduate deferral in July 1967 resulted in a larger relative decline in graduate school attendance by draft-eligible young men compared to draft-ineligible young women.

The unexpected policy change in 1967 had the potential to differentially affect the state of birth-college major cohorts we analyze. That is, based on the age and gender composition of certain cohorts (again, defined at the paired state of birth and college major level), a positive shock to research labor supply likely occurred virtually independent of unobserved factors affecting knowledge production. In fact, our "first-stage" analysis bears out this prediction: labor markets more exposed to the draft – i.e., because of a disproportionate share of draft-eligible aged men – experienced a larger increase in the supply of researchers once graduate school draft deferrals were no longer an option.[5] The effects are large and precisely estimated. The average state of birth-college major cohort experienced a 26% increase in the number of researchers as a result of the end of graduate school draft deferrals.

Evaluating how this labor supply shock impacted knowledge production, our analysis reveals that the research effort effect depends on the type of knowledge produced. Academic publications display a unitary effort effect, thus providing little evidence in favor of duplication or agglomeration effects in the aggregate. For patents we find an effort elasticity near zero, indicating a strong duplication effect. We find a negative intertemporal effect reflecting a so-called "fishing-out effect" – i.e, novel ideas become harder to find as the stock of knowledge grows. We find that fishing-out effects are very similar across types of knowledge produced and researchers.[6]

[4]Our paper is not the first to study how Vietnam draft eligibility affected scientific labor supply. Contemporary work that sought to predict the supply of scientists often included measures of draft eligibility in their analyses. See Ehrenberg (1992) for a review of this literature.

[5]A large literature following Angrist (1990) has sought to use randomly assigned Vietnam lottery draft numbers to estimate the impact of military service on later life outcomes. Two points are worth noting. First, in contrast to Angrist (1990) we study the response to a change in draft risk – i.e., the 1967 elimination of the graduate school draft deferral – that occurred before the draft lottery was introduced in 1969. Second, our estimated effect of changes in draft risk incorporates any effect the policy change had on military employment that may have further impacted a STEM-trained worker's involvement in research.

[6]That we find similar fishing-out effects for both patent and academic publications echoes Sampat and Williams (2019) who show that negotiation over IP rights does not reduce follow-on innovation, but contrasts

We explore our approach's credibility in a few ways. First, we examine whether researcher supply or knowledge production effects appeared before the draft deferral policy change in 1967. Our approach would be undermined if the draft deferral policy itself was driven by unobserved changes in knowledge production. Next, we test whether our results are sensitive to the level of aggregation we implement in defining research cohorts based on state of birth-college major pairs. Our baseline approach may underestimate the effect of knowledge stocks on innovation if knowledge flows across our defined cohorts are correlated with demographically-driven draft exposure. Finally, we examine whether the dynamics of the effects on knowledge production are consistent with decreasing returns (i.e, "fishing-out"). Short term effects being much smaller than long term effects would be challenging to square with an intertemporal fishing-out interpretation. We find little evidence from any of these analyses that our main findings and conclusions are not robust.

In the final section of the paper we study how the shock to researcher supply affected firm productivity. By directly estimating the returns to scale from non-rival ideas, we can build off of prior work that uses the semi-endogenous growth model to determine the social rate of return to R&D (Jones and Williams 1998).[7] We use firm identifiers in the NRSTP dataset to build a measure of exposure to the end of draft deferral for each firm and compute revenue-based Total Factor Productivity (TFPR) using Compustat data. We find that more exposed firms increased their research effort and TFP, implying a TFP-researcher scale elasticity of 0.23 or 0.39. These results imply a social rate of return to R&D of 36% to 56%.

We see our study as making six contributions to the literature. First, this research is closely connected to recent work that seeks to understand the effects of skill upgrading and immigration on innovation.[8] Other work has emphasized the importance of tal-

with Williams (2013) and Galasso and Schankerman (2015) who show that IP rights can reduce follow-on innovation for complex technologies. While Furman and Stern (2011) show that institutions can play an important role in cumulative innovation, we find that intertemporal effects are very similar across institutional environments.

[7]The semi-endogenous growth framework fits the time series evidence well (Jones 1995) and connects closely to seminal Schumpeterian endogenous growth models such as Aghion and Howitt (1992) (Jones 2022b). The social rate of return in Jones and Williams (1998) results directly from the production possibilities of the economy: the production function for new ideas and the production function for the output good. It does not rely on any particular assumptions regarding market structure, the patent system, or distortionary taxes.

[8]Human capital (Waldinger 2016, Toivanen and Vaananen 2016, Bianchi and Giorcelli 2020) and reductions in entry barriers (Bell et al. 2019) directly increase an individual's innovative output. Incumbent researchers receive spillovers from star collaborators (Azoulay et al. 2010), co-located innovators (Moretti 2021), and immigrants (Borjas and Doran 2012, Moser et al. 2014, Bernstein et al. 2018, Moser et al. 2021). Our emphasis on how upper-tail human capital creates growth through new ideas contrasts with recent work focusing on technology adoption (Squicciarini and Voigtländer 2015) and occupational choice (Cantoni et al. 2018). See Van Reenen (2020) and Biasi et al. (2021) for recent literature reviews.

ent allocation for complementarity between human capital and innovation (Akcigit et al. 2020), as well as aggregate effects from inventor migration (Prato 2022). We contribute by providing a market-level estimate of the effect of marginal researcher supply that seminal semi-endogenous growth models (Jones 1995, Jones and Williams 1998) require to provide counterfactual policy estimates.

Second, we provide a direct estimate of the research effort effect. We find a unitary effect for academic publications, consistent with Romer (1990), but a duplication effect for patents. In this way our results may offer an explanation for why policies targeting basic research may have relatively large returns (Akcigit et al. 2020, Dyevre 2024). They are also consistent with winner-take-most patent races promoting R&D effort duplication (Loury 1979, Dasgupta and Stiglitz 1980, Lee and Wilde 1980). Recent work on the effects of historic immigration shocks on patenting and growth in US counties (Terry et al. 2024) finds a larger research effort effect that we do. Spillovers from immigrants onto US innovators likely differed from those experienced among domestic researchers in our context.

Third, by providing a cross-sectional estimate of intertemporal spillover effects, we contribute to the debate surrounding why innovation has become harder (Jones 2009 and 2010). There are two possibilities. Innovation may have gotten harder over time because ideas are being fished-out over time. Alternatively, a secular decline in research productivity may have led the knowledge production function to shift. The gap between our intertemporal spillover effect and the time-series estimates in Bloom et al. (2020) suggests that only 20% to 55% of the decline in innovative output is accounted for by secular changes in the knowledge production function.

Fourth, we provide an estimate of the returns to scale from non-rival ideas that Jones (2022b) notes remains an open question in the literature. Peters (2022) estimates returns to scale looking at the effects of exogenous refugee-driven population changes on growth. By estimating the effect of an exogenous shock to the supply of researchers directly, we obtain a non-rival ideas return to scale elasticity that is directly applicable to an ideas-based semi-endogenous growth framework.

Finally, we offer a new estimate of the social returns to R&D. While the semi-endogenous framework in Jones and Williams (1998) has long been influential, the lack of both knowledge production function and goods production function parameter estimates needed to obtain a rate of return have limited its application.[9] By utilizing our research effort-intertemporal

[9]Jones (2022b) and Jones and Summers (2021) point to the scarcity of estimates outside of the seminal contribution of Bloom at al. (2013) as an important gap in the literature.

spillover-returns to scale estimate triplet, we harness the Jones and Williams (1998) framework to provide a new estimate of the social return to R&D. Ideas are getting harder to find, which reduces the social rate of return to R&D, only marginally. Even though ideas might be getting harder to find, our results indicate that social returns to R&D remain large.

2 Historical Background

The Vietnam War Draft. As US military involvement in Vietnam increased significantly in 1965, millions of draft-age men (i.e., 18 to 25 years old) faced critical and potentially life-changing choices.[10] The fear of being drafted and risking death in combat, the personal costs – including opportunity costs – of serving, one's opposition to the geopolitical wisdom or the morality of American involvement in Vietnam, and the uncertainty of the institutional nuances of the draft and local Selective Service boards' decision-making all weighed heavily on young men's minds.[11] While, statistically, the odds that a man at risk of being drafted during the Vietnam era would actually see combat in Vietnam was relatively low, the escalation of the war by 1966 and the public turmoil over it, marked a turning point in the salience of the draft in American life especially as the number of inductions (i.e., drafts) tripled from 1965 to 1966.[12]

For men who were committed to avoiding the draft, for whatever reason, or their country's

[10]There were approximately 23,300 US military personnel in Vietnam in 1964. The next year that number had escalated to 184,300, and increased to a peak of 536,100 in 1968. See US Bureau of the Census (1977, 369).

[11]While the Selective Service System followed federal law, the actual system was maintained through the work of about 4,000 local boards across the country that classified registrants in the Selective Service into various categories that determined which men would ultimately fulfill national induction calls. As the Yale Law Review (1966, 178-9)) concluded, "For the registrant, seeking to find where he stands with the draft, Selective Service is thus a maze of 'advisory' federal policies, enforced, modified, or ignored by his local board. . . . [D]espite the system's quasi-national character, registrants with similar backgrounds are treated wholly differently by different boards." Yet, according to Lewis B. Hershey (1966, 4), Director of the Selective Service System, local control was necessary. "The people of the country will support a compulsory system only to the extent that they have confidence in its fairness and they will have confidence in a system only to the extent which they themselves operate it." See also Wilson (1966) for an excellent discussion of the administrative vagaries of a Selective Service system that sprang from the emergency of World War II and met the Vietnam emergency largely unchanged.

[12]Among all active duty military personnel in any given year, only a maximum of about 18 percent were sent to the Vietnam theater. Draftees never constituted more than 30 percent of total military personnel during the Vietnam era at its peak. In fact, the vast majority of military personnel were volunteers, though it was certainly the case that some men were induced into voluntarily enlisting, rather than face the risk of being drafted, to have better control of their eventual fate during their military service. In sum, among all men of prime draft age (i.e., 18 to 25 years old) registered with the Selective Service during the Vietnam era, no more than three percent in any given year were involuntarily drafted into the military. These calculations are based on Selective Service statistics published in the *Annual* and *Semi-Annual Report of the Director of Selective Service* during the 1960s. Vietnam conflict personnel data are from US Census Bureau (1977, 369).

involvement in Vietnam altogether, a number of Selective Service policy changes during the 1960s certainly gave the impression that proverbial windows were closing. For example, in anticipation of increasing calls for inductions and as part of a campaign to compel educational institutions to report on the status of their students, in January 1964 the Selective Service issued Operations Bulletin No. 252 that compelled local boards to expedite the classification of men who could otherwise avoid such classification for sometimes up to five years by claiming they were students (US Selective Service System 1964, 12-13). This classification effort bore immediate fruit: in 1963, for instance, the number of unclassified men among Selective Service registrants was 2.3 million; in 1964 it was down to 699 thousand; and by 1965 it had fallen to 213 thousand. Thus, for men claiming a student deferment, at least after 1963, their local boards had to make an active justification for the classification.

Further, by Executive Order 11241, effective August 26, 1965, married men without children were placed in the same draft order as their unmarried counterparts. Since 1963 married men had enjoyed a sequence ordering that put them behind the unmarrieds. According to the Director of the Selective Service, the policy change "was considered necessary because of the increased number of men being delivered for induction."[13] It is also worth pointing out that in response to what the Director considered "a year of protest against the war in Vietnam, against the draft and the like," Congress passed legislation outlawing the burning of draft cards or other Selective Service registration documents (Public Law 89-152, approved August 30, 1965). The Director found it "ironic" that the energy of the protest movement came from college students, especially given that 1.8 million were deferred from service anyway (US Selective Service System 1966, 1.)

Higher education proved to be a contentious sticking point in the debate over the draft as the war expanded. A fundamental function of the Selective Service since its major reform in 1951, beyond providing a means to provide adequate personnel for national defense, was to "channel" registrants into activities or occupations that served the broader interests of the country. From the Selective Service System's (1965, 18) perspective,

> "channeling" refers to that process through which registrants are influenced to enter and remain in study, in critical occupations, and in other essential activities in the national health, safety, and interest by deferment or prospect for deferment from military service. Selective Service channels thousands of young men through its deferment procedures into those fields of endeavor when there are shortages

[13]See US Selective Service System (1966, 11). For an empirical analysis of this abrupt policy change on marriage decisions in August 1965, see Bailey et al. (2021).

> of adequately trained personnel. Many would not have continued their education at the undergraduate and graduate college level if there had not been a college student deferment program.
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> Many younger engineers, scientists, technicians, and other skilled workers have been kept in their jobs through occupational deferments. Young male teachers are induced to remain in the teaching profession through deferment, and additional students are attracted into the profession. The fields of medicine and dentistry also have benefited from student and occupational deferment channeling.

As Cihlar (1969, 55) noted, "Scarce notice of this situation [channeling] was taken, however, until 1965. The basic unfairness of a system which tended to burden those without the resources or talent to go on to higher education was not so keenly felt at a time when military service did not entail any serious risk of combat."

End of Graduate School Draft Deferment. As the Universal Military Training and Service Act, which authorized the induction of men into the military, was set to expire on June 30, 1967, President Lyndon Johnson (1967, 3) appointed the National Advisory Commission on Selective Service (NACSS) "to review the procedures by which some men are selected and some are not." Among the numerous factors that Johnson asked the Commission to consider in its recommendation, foremost was "Fairness to all citizens." In fact, the importance of the issue was embodied in the title of the Commission's report submitted in February 1967 – "In Pursuit of Equity."

Among the numerous reforms that the NACSS (1967, 4-8) recommended, a few would represent significant departures from existing practice: the elimination of all educational and occupational deferments; switching from an "oldest first" order of induction to "youngest first" beginning at age 19; and the use of a lottery to decide the order of induction, which President Johnson (1967, 4) considered a "FAIR" (i.e., fair and impartial random) system. While President Johnson was largely supportive of the NACSS's recommendations, the complete abolition of college and occupational deferments was perhaps a bridge too far. In transmitting his recommendations for modifications to the Selective Service to Congress, Johnson was clear on his view of college deferments, which he concluded was the "most difficult problem" demanding attention. He argued that "Student deferments have resulted in inequities because many of those *deferments* have pyramided into *exemptions* from military service. Deferred for undergraduate work, deferred further to pursue graduate study and then deferred even beyond that for fatherhood or occupational reasons, some young men have managed to pile deferment on deferment until they passed the normal cutoff point for

induction . . . . *To correct the inequities in the deferments of postgraduate students*," Johnson called for the elimination of deferments for graduate students, except for those pursuing medicine or dentistry (Italics in original; Johnson 1967, 7-8).

The Military Selective Service Act of 1967, which renewed the Selective System within the US, made only modest progress in removing the alleged "inequities" that the President and NACSS hoped to alleviate. The most significant change to the Selective Service in 1967 focused on the elimination of automatic postbaccalaureate deferments, "except for extreme hardship to dependents . . . or for graduate study, occupation, or employment necessary to the maintenance of the national health, safety, or interest" (Public Law 90-40 § 6(h)(1)). The legislation delegated to presidential determination what fields were actually "necessary," which the National Security Council ultimately decided was only medicine, dentistry, and other allied medical fields. Moreover, the list of essential activities and critical occupations that previously served as a guideline to local boards was suspended, thus leaving "essential community need," as determined by local draft boards, as the only basis for an occupational deferment.[14] Moreover, Congress maintained the status quo with regard to the sequencing of inductions, which effectively retained the "oldest first" ordering without lottery. Two years later Congress passed and President Nixon signed into a law an amendment to the Military Selective Service Act that authorized the draft lottery system and a "youngest first" ordering (Public Law 91-124, November 26, 1969).

Impacts of End of Draft Deferment. The Johnson Administration formally abolished draft deferments for graduate students and "critical occupations" on February 16, 1968. "While the local boards continued to have discretion to grant deferments as they saw fit, [Selective Service Director] Hershey's recommendations generally were expected to be followed" (Congressional Quarterly Service 1968, 1). Ending such student deferments and maintaining the "oldest first" induction order, clearly meant that college graduates who would have otherwise attended graduate school were now much more vulnerable to the draft. The private agency Scientific Manpower Commission (1968) predicted that beginning in July [1968], "almost the entire draftable population will have achieved one or more college degrees. The military forces will not be able to utilize a very high proportion of these men in officer programs. . . leav[ing] the bulk of the college graduate group with no choice other than to serve as enlisted men." Nelson (1968, 655), summarizing the status of graduate education in *Science* in November 1968, concluded that "for the man who has finished his undergraduate work, the Selective Service System is becoming less selective and putting more emphasis on

[14]For clear summaries on the policy changes implemented as a result of the 1967 revision to the Selective Service Act, see U.S. Office of Resources Analysis (1968, 15-16) and Fordham Law Review (1969, 622-23).

his obligation to do military service."

College graduates, without the fallback of entering graduate school for a lengthy extension of their deferment status, were nervous, creative, and sometimes willing to roll the dice. As Syracuse University alumnus Joel Swerdlow (1985, 19) remembered, "[A]s my class – 1968 – prepared to graduate, Vietnam was all we talked about . . . . In 1967, indifference became activism because graduate deferments ended. Suddenly, Vietnam was no longer someone else's war. You could get killed there." William Casement (2020), also a 1968 college graduate, would have been considered a "gambler" in the parlance of the time. He was offered a high school teaching job – an occupation that would have likely garnered him an occupational deferment – and an assistantship for a master's program in student personnel services. He wanted to pursue a career in higher education, so took his chances by enrolling in graduate school and risked being draft. He was called up mid-semester in 1969. Princeton University first-year graduate student Stephen Gelbart (2004, ix) found his own creative way to avoid the draft. He recalls that "Rather than go and fight, most of the students in my year [1967-1968] at Princeton had left the university to find acceptably deferable employment elsewhere. I was lucky enough to become an Instructor at Rutgers-Newark." With the end of graduate deferment, the benefits associated with pursuing a graduate degree diminished, at least immediately, and Figure 2 indicates the expected response. The STEM-trained workers represented in the NRSTP dataset, as we show below, flooded the labor market, instead of graduate programs, where many contributed to the country's research enterprise.

3 Data and Descriptive Statistics

This paper uses newly constructed datasets on the scientific workforce and knowledge production during the Vietnam War era. Our measurement relies on four main data components: (i) the National Register of Scientific and Technical Personnel, enabling us to identify scientists' demographic characteristics and the nature of their participation in the workforce; (ii) academic publication data that we link to the scientists to measure basic knowledge production; (iii) patent data that we link to the scientists to measure commercial knowledge production; and (iv) firm TFP data that we link to scientists to measure goods productivity. In this section, we briefly describe the construction of these components and some data limitations. Detailed discussions of the construction of each variable, as well as the data sources, are available in the online appendix sections 1 and 2.

Scientific Personnel. The core data employed consist of information on individual scientists from the National Register of Scientific and Technical Personnel (NRSTP). A

particular strength of the data is that the same scientist can be observed over multiple years. Our main dataset covers the years 1960, 1962, 1964, 1966, 1968, and 1970.[15] The roster was populated by sending mail surveys to membership lists of major national scientific societies.[16] Participation in the surveys was voluntary and the response rate was typically around 65%.[17] Some questions in the surveys differed from year to year, though the number of questions generally increased from 1954 to 1970. The data include the respondent's name, a range of demographic information, such as age, gender, and date and place of birth, educational attainment, present geographical information, work activity (e.g., basic or applied research, management, teaching), and employer name. We match scientists across the 1960 to 1970 survey years using the scientist's reported name, educational profile, current location, and date of birth. The matching is flexible enough to account for differences in any of the matching variables. These differences might include spelling variations, such as reporting first names "WILL" and "WILLIAM" on different surveys, a change in the highest degree attained, or moving to a neighboring city. The resulting panel contains 555,279 unique scientists observed between 1960 and 1970. The construction of the scientist panel is described in more detail in the data appendix, section 2.1.

Patent and Publication Linkage. We link scientists with disambiguated author profiles in the Microsoft Academic Graph (MAG) and disambiguated U.S. patent inventor profiles from Google Patents (GP) to obtain annual counts of publication and patenting measures for individuals in our data. We successfully link 252,420 (or 45%) of the unique individuals represented in the dataset to an author profile containing publications in the MAG, and 104,015 (or 19%) with a GP inventor profile. Details of the construction of the patents and publications data and of the linkage of these data with scientist records are available in the data appendix, sections 2.2-2.4.

Draft Exposure. Our research design is based on comparing differences between mid- versus early-career researchers, before and after the end of graduate school draft deferral, across state of birth-college major pairs with relatively high- and low-draft exposure. We define "early career" here as the first 10 years after the typical age of graduation from an

[15] Two additional iterations of the NRSTP were conducted in 1954 and 1958. However, the list of variables and level of detail of each variable changed dramatically after these two early periods. Our analysis only employs the post-1958 data, though our linkage procedure is also applied to the pre-1960 data.

[16] Eight societies participated in all survey years: American Institute of Biological Sciences, American Chemical Society, Federation of American Societies of Experimental Biology, American Geological Institute, American Institute of Physics, American Mathematical Society, American Meteorological Society, and the American Psychological Society.

[17] Only respondents meeting certain criteria for "full professional standing" were included in the Register. The criteria, for each professional association, are described in Appendix C of the 1970 report *American Science Manpower* (National Science Foundation 1971).

undergraduate degree in the US. Those individuals aged 32 to 41 are defined as "mid-career." We drop those who were aged 42 or older from our analysis. Each comparison group is, thus, defined at the state of birth, undergraduate major, and cohort level. An example grouping would consist of those born in New York who completed their undergraduate major in Physics and who were younger than age 32. Our group-level draft exposure measure is based on the fraction of early career workers enumerated in the NRSTP whose age and gender made them potentially draft-eligible in 1966.[18] Specifically, we measure draft exposure in a state of birth-college major pair, m, in 1966 as:

$$\text{Draft Exposure}_{m,1966} = \frac{\sum_{i=1}^{n} \text{Male Less Than Age 26 in 1966}_{im}}{\sum_{i=1}^{n} \text{Individual Less Than Age 32 in 1966}_{im}}, \tag{1}$$

where i indexes individuals in the NRSTP data. This measure captures the fraction of the overall early-career cohort who could conceivably be drafted (i.e., men younger than age 26) among all those who were born in a particular state and who majored in a specific discipline during college. The measure is taken from 1966, the last full year before the policy change.

Figure 3 shows the distribution of draft exposure across groupings, by undergraduate major regardless of state of birth. We see that undergraduate majors such as Physics, Meteorology, and Chemistry had high levels of exposure. Undergraduate majors such as Psychology, Health, and Linguistics had much lower levels of exposure. Put another way, the majors that faced the greatest draft risk exposure were the ones that had a disproportionate share of younger men (i.e., under 26 years old). Figure 4 presents the spatial distribution of draft exposure by state of birth, regardless of college major. High-exposure states included California, Michigan, and Missouri; low-exposure states included New Hampshire, Arizona, and Minnesota. The map shows that high-exposure states were distributed throughout the country, with perhaps a higher frequency in the Mid-West. In appendix figure A4 we see that there is variation in draft deferral exposure within majors. Those born in Illinois who receive an undergraduate degree in Chemistry face high draft deferral while those born in Illinois whose undergraduate major is in Health face low draft deferral exposure, for example. In Section 4 below we show econometrically that our measure of 1966 draft exposure performs well in explaining how and where the research workforce expanded as a result of the 1967 policy ending graduate school deferment during the Vietnam War.

Sample Selection and Descriptive Statistics. Our analysis is conducted using ag-

[18] Given the information available to us, we must ignore factors that would have significantly lessened an individual's risk of draft, or "exposure." For example, if a man had dependent children, was physically or mentally unfit, or was so persuasive or politically connected that his local draft board would have deemed his specific job in the national interest, he was more likely to secure a draft deferment.

gregated data at the state of birth-undergraduate major-age grouping level. We consider two age groups - early-career (ages 22-32) and mid-career (ages 33-42) - defined based on age in the year of the survey. For example, one observation in our analysis might consist of all scientists under 32 from California who majored in Chemistry in college. This is useful because only the early-career cohort is directly exposed to the change in draft deferral policy that affects males age 26 or younger. We measure draft deferral at the state of birth-undergraduate major for those less than age 32. Our treatment variable is the interaction between this state of birth-undergraduate major exposure $\times$ the early-career indicator and our model is identified based on the differential effect of draft exposure between early- and mid-career age groups.

As the draft exposure measure is constructed from the NRSTP, measurement error in the variable may be significant for groupings with few NRSTP observations. To address this issue we drop observations with less than 10 NRSTP respondents in 1966. The analysis sample contains 4,928 state of birth-college major-cohort group observations between 1960 and 1970, encompassing combinations of 50 non-missing states of birth and 17 non-missing undergraduate majors.

Table 1 provides a first look at summary statistics of relevant measures in 1960, the first year of NRSTP data. Column (1) presents the means and standard deviations of key variables for the full sample. We first stratify the group-aggregated data based on cohort. The early-career cohort – ages 22 to 31 – were directly affected by the draft deferral policy change and the mid-career cohort – ages 33 to 41 – were not. In columns (2) and (3) we stratify based on whether an early career cohort had 1966 draft exposure, as defined in Equation (1), that was above or below median. Column (4) reports the p-value for differences in the baseline variables for the early-career cohort. In columns (5) to (7) we conduct the same analysis where we stratify the mid-career cohort based on whether the draft exposure of their early-career counterparts with the same state of birth-college major grouping was relatively high or low. In column (8) we report p-values for the difference between the differences in the baseline values.

Columns (2) to (4) show that within the early-career cohort, groups that would later be more exposed to the end of graduate draft deferral in 1967 were quite different from less-exposed groups in 1960. In columns (2) and (3) we see that among the early-career cohort, groups that would eventually be more heavily exposed to the end of graduate draft deferral generally had lower research inputs, output, and earnings, as well as more university, less private-focused employment, and less military-funded projects. The results in columns

(5) to (7) show similarly that baseline differences existed across a range of variables among the mid-career groups. Turning to column (8), however, we only see statistically significant baseline differences between the two differences for patents. Table 1 provides evidence, then, that triple difference treatment and control groupings were quite balanced in 1960 before the US entered the Vietnam War.

4 Knowledge Production

Our goal is to estimate the parameters of the knowledge production function $\dot{A}_{bat}$ given by:

$$\dot{A}_{bat} = e^{\epsilon_{bat}} R_{bat}^{\lambda} A_{bat}^{1-\beta}, \tag{2}$$

where $\dot{A}_{bat}$ is the idea production in birth state-undergraduate major group b, age group a, at time t, R_{bat} is the number of researchers in birth state-undergraduate major group b, age group a, at time t, and A_{bat} is the stock of knowledge within birth state-undergraduate major group b, age group a, at time t.[19] Again, a group in our context refers to group of scientists all born in the same state and majoring in the same college discipline. For each birth state-undergraduate major group b we consider two age groups - early career and mid career. The *Research Effort* elasticity is λ. When there is no agglomeration or duplication of effort, then $\lambda = 1$. Duplication implies $\lambda < 1$; agglomeration implies $\lambda > 1$. The *Intertemporal Spillover* elasticity is $1 - \beta$.[20] A "standing-on-shoulders effect" implies that $\beta < 1$, so that the flow of new ideas increases with the existing stock of knowledge. A "fishing-out effect," where the flow of new ideas declines with the existing stock of knowledge, occurs when $\beta > 1$.

4.1 Two-Step PPML Triple Differences

To estimate the two parameters of the knowledge production function, we follow a two-step reduced form approach using a triple differences design. The first step is to estimate how the graduate draft deferral policy change affected the number of researchers within an observed group. The next step estimates how the policy change affected publication and

[19] Our baseline approach uses the group level knowledge stock to quantify β. Researchers may draw from the knowledge stock of those born elsewhere to generate new knowledge. If the importance of building off knowledge elsewhere is systematically related to the size of a group's knowledge stock our estimates could be biased. To explore this possibility we also estimate our models at higher levels of aggregation and find that our results change little.

[20] There are multiple ways to specify the intertemporal spillover elasticity. Jones (1995) specifies the knowledge production function as $\dot{A}_{bat} = R_{bat}^{\lambda} A_t^{\phi}$, where $\phi < 1$. We follow Jones (2022a) in specifying $\beta \equiv 1 - \phi$, as this formulation allows a close mapping to the innovation rate in the quality ladder model of endogenous growth in Grossman and Helpman (1991) and Aghion and Howitt (1992).

patent outputs within that group five years later. By utilizing the reduced form effects of the end of graduate school draft deferral on both the number of researchers and knowledge outputs, we can obtain estimates of the research effort effect (λ) and the intertemporal effect (β).

We seek to capture both intensive and extensive margin responses by including observations with zero values in our sample. Including these observations means that we are unable to estimate a log transformed knowledge production function represented in equation (2). It is possible to adapt a log-transformation utilizing an inverse hyperbolic sine (IHS) transformation for the outcome variable to include zero observations. However, given scale variance concerns with IHS (Chen and Roth 2024) we instead adopt a Poisson Pseudo Maximum Likelihood (PPML) approach that preserves the multiplicative structure of the knowledge production function rather than linearizing it. We recognize that estimating triple differences models in a multiplicative functional form may be unfamiliar, but we view both potential margins of response to be important in estimating the knowledge production function.

Reduced Form Researcher Effect. We first use PPML to estimate the reduced form effect of the end of graduate school draft deferral on researcher supply as:

$$R_{bat} = e^{(\pi_b+\pi_a+\pi_t+\pi_{ba}+\pi_{bt}+\pi_{at}+\psi_{RF}\text{Early Career}_b\times\text{End Deferral}_t\times\text{Draft Exposure}_{b,1966})}\mu_{bat}. \quad (3)$$

R_{bat} is the number of researchers in state of birth-college major group b, age group a, and year t. π_b are birth-college major group fixed effects, π_a are age group fixed effects, π_t are year fixed effects, π_{ba} are birth-college major group times age group fixed effects, π_{bt} are birth-college major group times year fixed effects, and π_{at} are age group times year fixed effects. Our specification controls for time invariant factors at the birth-college major group and age group level, as well as allowing for flexible time trends at the birth-college major group and age group levels. μ_{bat} is the error term assumed to be statistically independent of the regressors so that $E(\mu_{bat}) = 1$.

Our parameter of interest is ψ_{RF} which captures the reduced form effect of the policy change shock on the early career age group relative to the mid career age group. We expect $\psi_{RF} > 0$ as the end of graduate draft deferral likely led to an influx of researchers in the state of birth-major groupings where the younger cohort was relatively more exposed to the draft. We cluster the standard errors at the group level in all our models.

Exposure to the draft could affect the number of researchers in the labor force in two ways. The main effect was likely draft-exposed individuals' direct response to the end of

the graduate deferral. Since graduate school attendance no longer provided a refuge from being drafted, thus reducing the benefit of attaining an advanced degree, many of these STEM-trained men no doubt sought to join research labor force. Yet a secondary effect could also be important in that individuals facing high draft risk may have been more likely to voluntarily enlist in the military (Oi 1967). Any measurable effects on research activity associated with entering the military as a scientist will be picked up in our main estimates as we do not condition the sample to include only civilian researchers. Since the knowledge production function in the military sector may be quite different from the non-military sector, we estimate our models on a sub-sample of private sector and university-employed researchers in a robustness check. Accounting for this nuance in sectors does not change the main findings.

Reduced Form Knowledge Production Effects. We then use PPML to estimate the reduced form effect of the end of graduate school draft deferral on knowledge production as:

$$\dot{A}_{bat+5} = e^{(\theta_b+\theta_a+\theta_t+\theta_{ba}+\theta_{bt}+\theta_{at}+\delta_{RF}\mathrm{EarlyCareer}_a\times\mathrm{EndDeferral}_t\times\mathrm{DraftExposure}_{b,1966})}u_{cmt}. \quad (4)$$

$\dot{A}_{bat}$ is knowledge output (publications or patents) in state of birth-college major group b, age group a, and year $t+5$, θ_b are the birth-college major group fixed effects, θ_a are age group fixed effects, θ_t are year fixed effects, θ_{ba} are birth-college major group times age group fixed effects, θ_{bt} are birth-college major group times year fixed effects, and θ_{at} are age group times year fixed effects. Our specification controls for time invariant factors at the group-cohort level as well as allowing for flexible time trends at the group and cohort levels. u_{bat} is the error term assumed to be statistically independent of the regressors so that $E(u_{bat}) = 1$.

Our parameter of interest is θ_{RF} which captures the reduced form research productivity effects five years after the event on the younger cohort within groups more exposed to the draft. We expect $\delta_{RF} > 0$ if the influx of researchers in the early career cohort led to more knowledge production. We cluster the standard errors at the state of birth-college major group level in all models.

Research Effort and Intertemporal Spillover Effects. We obtain the research effort effect, λ, by taking the ratio of the reduced form coefficient for knowledge production to the reduced form coefficient for researcher supply (i.e., $\lambda = \frac{\delta_{RF}}{\psi_{RF}}$).

Next, we derive the intertemporal spillover effect, β. The production function model expressed in equation (2) implies that the marginal effect of the reduced form shock for each

knowledge outcome – either publications or patents, depending on specification – is:

$$\frac{\partial \dot{A}_{bat}}{\partial \text{RF}} = e^{\epsilon_{mt}} \lambda R_{cmt}^{\lambda-1} A_{bat}^{1-\beta} \frac{\partial R_{bat}}{\partial \text{RF}}. \tag{5}$$

Denoting our estimated expected value of $\frac{\partial \dot{A}_{bat}}{\partial \text{RF}} = \widehat{MA}$ and $\frac{\partial R_{bat}}{\partial \text{RF}} = \widehat{MR}$ we can solve for the expected β as

$$\hat{\beta} = 1 - \Big[\frac{log(\widehat{MA}) - log(\widehat{MR}) - log(\widehat{\lambda}) - (\widehat{\lambda} - 1)log(R_{bat})}{log(A_{bat})}\Big]. \tag{6}$$

To obtain an estimate of β, we substitute the empirical analogs into equation (6) for each knowledge production outcome, either publications or patents. For $\widehat{MA}$ we use the estimated marginal effect of the draft deferral policy shock on each respective knowledge production outcome. For $\widehat{MR}$ we use the estimated marginal effect of the policy shock on researchers. For $\hat{\lambda}$ we use the ratio of the reduced form effect of knowledge production over the reduced form effect for researchers. For R_{bat} we use the average number of researchers before the policy change; and for A_{bat} we use the total stock of both patents and publications in the pre-policy change period as new patents and publications could each draw on both types of knowledge.[21]

Research Design. The central estimation challenge is that researchers' labor supply, R_{bat}, may be endogenous as researchers do not invest randomly. Because some components of the knowledge stock are unobserved to the econometrician – e.g., trade secrets or non-patented innovation – naive correlations between knowledge production and research effort likely yield a biased estimate of λ. We would expect that a naive correlation would yield an upward biased estimate. Researchers with more information than an econometrician would supply more effort when there is a positive research productivity shock.

To overcome the challenge of estimating an unbiased λ, we use the end of graduate school draft deferral during the Vietnam War as an exogenous source of variation in the potential supply of young male researchers participating in the idea-creation process. After 1967, when graduate school was no longer a viable means to defer one's draft eligibility, those facing draft risk would have been less likely to pursue a graduate education, all else equal. Our main source of exogenous variation, then, is based on demographically driven variation in the size of the feasible draftee cohort across state of birth-college major pairs in 1966. Those pairs where the young cohort had a relatively larger fraction of males less than age

[21] This equation is not solvable for cases where the knowledge production effect is strictly positive (i.e., $\widehat{MA} < 0$ or $\widehat{\lambda} < 0$). To approximate $\hat{\beta}$ in these cases we set $log(\widehat{MA}) = 0$ or $log(\widehat{\lambda}) = 0$.

26 would have presumably experienced a larger shock to its supply of potential researchers; those with fewer young males would have received a smaller shock.

Importantly, this shock to the supply of researchers is unrelated to unobserved determinants of research productivity. Thus, we can obtain an unbiased estimate of λ – the research effort elasticity – free from concerns about bias from unobserved researcher productivity confounders. With an unbiased estimate of λ in hand, we use the structure of the knowledge production function to obtain an estimate of β – the intertemporal spillover elasticity.

A state of birth-college major group's exposure to the draft could have been related to exposure to military activity directly. A possible concern is that we could be picking up some of the direct effect of changes in military activity that is correlated with draft deferral policy changes across groups. For example, men working for military contractors may have had a stronger chance to secure an occupational deferment from their local draft boards. Our triple difference design using cross-cohort variation and controls for group $\times$ year fixed effects flexibly controls for changes that may have occurred at the group level. Further, to address the concern that direct effects could differentially occur at the group $\times$ cohort level, we add two additional controls to our models. By adding controls for the fraction of scientists receiving military funding in 1966 and military employment in 1966 within each cohort-group $\times$ year fixed effects, we are able to capture time series changes in military activity that may have been correlated with group $\times$ cohort draft risk.

Our research design is based on the idea that state of birth-undergraduate major groups with different levels of draft exposure (i.e., because of a relatively greater share of draft-eligible men under 26 years old) before 1967 did not experience positive shocks to levels of young researcher supply until after the draft deferral for graduate school was eliminated. We regard this assumption as plausible given that the decision to end the draft deferral for graduate school was made only after accelerating US military involvement in Vietnam and equity concerns became paramount. In addition, as exposure to the change in draft deferral was heterogeneous across majors and states of birth – see Figures 3 and 4 – our research design harnesses the group-specific nature of the shock. Of course, while unlikely, it is possible that unobserved cohort-specific trends in knowledge production in high- versus low-exposure groups drove the decision to end draft deferral. To examine this potential threat to our identification strategy, in later analyses we empirically explore the possibility that the end of draft deferral may have been in response to pre-existing trends in the knowledge production sector at the state of birth-college major group level.

4.2 Results

Main Results. The results of estimating equation (3) using total number of researchers and equation (4) using publications and patents (as well as a log transformed OLS model of labor earnings) as outcome variables are reported in Table 2. The table reports mean marginal effects from the PPML models and coefficient estimates for the OLS models (for log (earnings)).[22]

Columns (1) and (2) show that once graduate deferrals were disallowed, the number of researchers increased in the young cohort groups where that group faced greater exposure to draft risk. The estimates are statistically precise and economically meaningful. The estimation in Column (1) implies that the average young-cohort group experienced a 26% increase in the number of researchers as a result of ending graduate school deferrals.

In columns (3) and (4) we find negative but imprecise point estimates on labor earnings consistent with labor demand for researchers being elastic.[23] Columns (5) and (6) show precisely estimated increases in publications, while columns (7) and (8) reveal smaller and imprecise estimates of the marginal effect of eliminating graduate school deferrals on patents.

The research effort effect results suggest little duplication effect for publications as λ is close to 1. In contrast, our estimates for λ are much smaller for patents, indicating substantial duplication effects. Both the patent and publication estimates show a moderate fishing-out effect for knowledge production, with β between 1.34 to 2.38.[24]

Prior Trends. A potential lingering concern is that ending the draft deferral for graduate school may have been endogenous to underlying trends in researcher supply dynamics. Or, perhaps, the graduate draft deferral policy change was related to unobserved shocks in knowledge production in highly draft-exposed markets. While our reading of the historical evidence indicates that the draft deferral policy change was driven by the imperative to

[22]As we are estimating our models using PPML observations will be dropped from the sample if there is no within variation conditional on the fixed effects we estimate. This can lead to different samples across the outcome variables. To retain comparability in the samples across outcome variables we only keep observations that are retained in both the researchers and publications econometric models. As our measure of patents is more likely to contain zero values the PPML procedure retains a smaller sample that for the other outcomes. For this reason we do not condition out sample on an observation having to be in our patent estimation sample for our main analysis. We show the robustness of our estimates to also using a sample that includes only observations included in the patent model estimation sample in online Appendix Table A5.

[23]The amenity value that scientists receive from working in a research position (Stern 2004) may also attenuate any wage effect.

[24]See Bloom et al. (2020, 1134) for a summary of estimates of β. Their estimate for the entire economy from 1930 to 2015, for example, is 3.1.

address inequality in military service and did not follow trends in knowledge production in specific locations or majors, exploring prior trends is an important specification check.

In Figure 5 we graphically present dynamic versions of our main econometric model with 1966 as the reference year.[25] The results from this analysis reveal little evidence of prior trends. The coefficients of the 1960, 1962, and 1964 interactions are very close to zero and not statistically different from zero at any conventional confidence level. These results lend additional credibility to our research design.

Robustness. Our main conclusions change little using alternative inference procedures (appendix table A3), using an alternative draft exposure measure (appendix table A4), restricting the non-patent samples to only those observations available in the smaller sample available when analyzing patents (appendix table A5), or when comparing various heterogeneous characteristics of the data, such as the impact of the publications or patents produced, types of employers considered, or types of researchers considered (appendix table A6). We also perform a falsification test, focusing exclusively on samples by gender (appendix table A7). Reassuringly, we find a null effect in using a sample of females, which is expected since they would not have been directly impacted by the changed deferral policy. A more detailed discussion of these robustness checks is available in the online appendix section 3.

Aggregation. In our analysis thus far we have defined a unit of observation at the state of birth–undergraduate major–cohort level. By defining the groups based on time-invariant characteristics, our approach effectively shuts down possible migration or reallocation responses. However, knowledge could flow across groups leading to spillovers that could cause our group-based approach to under or overstate aggregate effects.

To address this potential issue, in Table 3 we show results from estimating the models using alternative levels of group aggregation. In panel A we report our baseline state of birth-undergraduate major-cohort level analysis from Table 2 for reference. We then show results aggregating to the census region of birth-undergraduate major, census division of birth-undergraduate major, and nationwide- undergraduate major levels in panels B, C, and D, respectively.

Moving down column (3) shows that the estimates of the research effort and intertemporal

[25] Specifically, the model we estimate is:

$$dotA_{mct} = e^{(\pi_c+\pi_m+\pi_t+\pi_{cm}+\pi_{mt}+\pi_{ct}+\sum_{y=1960, y\neq 1966}^{1970} \pi_{y2}\text{Early Career}_c\times\text{Year=y}_t\times DraftExposure_{m,1966})}\mu_{cmt} \quad (7)$$

where $Year = y_t$ is a dummy variable that takes a value of one for year y and zero otherwise. The excluded year is 1966. Other variables are defined as in equation (2).

effects depend little on the level of aggregation for academic publications.[26] There is more variation in the research effort estimates for patents in column (4). The patent estimates are not monotonically related to the level of aggregation and likely reflect limited precision of the underlying estimates rather than cross-market spillovers that our baseline analysis may not have captured.

Short-Run versus Long-Run Effects. Our analysis thus far has utilized the structure of the knowledge production function to estimate intertemporal elasticity $1-\beta$. We do this by estimating effects that take 5 years to manifest - which we consider short run effects - and then utilize the structure of the knowledge production function to obtain implied intertemporal effects.

An alternative approach is to utilize the dynamics of the policy impact on researchers to estimate an intertemporal elasticity directly. The short-term effects would primarily capture the research effort response, while a longer-term effect would largely capture the intertemporal effect. If there were a standing-on-shoulders effect, then a positive intertemporal effect will amplify the research effort effect over time. In this scenario, the long-term effects of an increase in researcher supply will be substantially larger than short-term effects. On the other hand, if fishing-out effects are more prevalent, then negative intertemporal effects will dampen research effort effects over time. In this case the long-term effects of an increase in researcher supply would be expected to be substantially smaller than short-term effects.

We estimate the short and long-term effects using an event study. We structure our data so that the unit of observation is a calendar year for an age cohort within a state of birth-undergraduate major group. For each cohort we use our measure of exposure to draft deferral policy change in 1967 based on the share of the population subject to draft risk as above. We employ the same research design comparing directly exposed early career cohorts to late career cohorts. The difference is the use of cohorts based on the age at in 1967 rather than contemporary age.

To compare short- and long-term knowledge production effects we estimate the following model:

$$P_{bct} = e^{(\kappa_b+\kappa_c+\kappa_t+\kappa_{bc}+\kappa_{bt}+\kappa_{ct}+\kappa_{SR}\mathrm{E}arlyCareer_c\times\mathrm{S}hortTerm_t\times\mathrm{D}raftExposure_{b,1966}+\kappa_{LR}\mathrm{E}arlyCareer_c\times\mathrm{L}ongTerm_t\times\mathrm{D}raftExposure} \tag{8}$$

where Short Term$_t$ and Long Term$_t$ take a value of one for the years 1968 to 1972 (inclusive)

[26]The magnitude of the coefficients capturing the marginal effects of the different dependent variables change substantially as the baseline mean of the outcome variable is larger at higher levels of aggregation.

and 1973 to 1980 (inclusive), respectively, and zero otherwise. All other variables are defined as above. We estimate the models by PPML for patent and publications outcomes using annual group-level data from 1960 to 1980. To be specific, the outcome P_{bct} is the total number of publications or patents for those in birth state - undergraduate major group b, in cohort c in the year t. Our parameters of interest are κ_{SR} and κ_{LR}. Strong inter-temporal increasing returns implies that $\kappa_{LR} > \kappa_{SR}$; decreasing returns implies that $\pi_{LR} < \pi_{SR}$.

We can estimate both and short term effects of the policy shock on knowledge production using this model. To obtain an implied β we also need to know how the end of graduate draft deferral affected cumulative researcher supply. However, because data on the number of researchers after 1970 is not available directly estimating the long term effects of the policy change on the number of researcher is not possible. Instead we utilize two approaches to approximate the long-term researcher effect. A first approach is to assume that the shock to researcher supply we estimate above is permanent, we can use our results from Table 2 column (1) to obtain the cumulative supply effect.[27] A second approach is to employ the steady-state time path from Jones (2022) to examine the sensitivity of the researcher adjustment process to assumptions.[28] With the cumulative researcher supply effect for each time horizon, K_k, in hand we obtain β by numerically solving the equation: $A_k = (\hat{A_0}^{\beta} + \beta K_k)^{\frac{1}{\beta}}$.

We report the results in Table 4. We see that the short-term intertemporal spillover elasticity for patents and publications show a fishing-out effect. They are quite similar in magnitude to those in Table 2 which have the same lag length. We also see that the short-run β is larger than the long-run β consistent with fishing-out effects. The results change little whether we assume a permanent or steady state shocks to researcher supply.

5 Firms and Growth: Calculating the Social Returns to R&D

To calculate the social rate of return to research effort in a semi-endogenous growth setting (Jones and Williams 1998), we need to measure the degree of increasing returns to

[27] The cumulative supply effect for time horizon k, $K_{ba,k}$, we use is given by: $K_{ba,k} = \frac{\kappa_{RF}}{1.5} \times t_{ba,k}$, where $t_{ba,SR} = 2.5$ and $t_{ba,LR} = 7.5$ is the average of the time elapsed in the short-run and long-run eras. The fact that labor income is little affected by the shock to researcher supply may indicate that there would be little (unobserved) reallocation response over the long-term.

[28] The steady state expression for the researcher time path under exponential population growth at rate n for time horizon k is given by: $K_k = \frac{\hat{R_0}^{\hat{\lambda}}}{n}(e^{\hat{\lambda} n t_j} - 1)$, where $t_{SR} = 2.5$ and $t_{LR} = 7.5$ is the average of the time elapsed in the short-run and long-run eras, $\hat{R_0}$ is the same average of the number of researchers in a state of birth-undergraduate major group, $\hat{\lambda}$ is our estimated research effort effect and n is the population growth rate.

scale from non-rival ideas.

Set Up. Consider the single sector semi-endogenous growth model in Williams and Jones (1998). The final goods technology is given by

$$Y = A^{\sigma} K^{\eta} L^{1-\eta}, \tag{9}$$

where Y is output, K is the capital stock, and L is assumed to grow exogenously at rate $n > 0$. A central parameter is σ, which reflects ideas being non-rival.

The production function for new ideas takes the form:

$$\dot{A} = e^{\epsilon} R^{\lambda} A^{1-\beta}. \tag{10}$$

Because individual researchers are small relative to the total number of researchers, they view the production of research as taking place with constant returns to research effort R.

Solving the idea production function for A_t along a balanced growth path (denoted by $\star$) where the growth rate of ideas is g_A, then gives:

$$A^{*} = \left(\frac{e^{\epsilon}}{g_A}\right)^{\frac{1}{\beta}} R^{\frac{\lambda}{\beta}}. \tag{11}$$

The level of output on the balanced growth path, in turn, can be expressed as:

$$Y^{*} = \left(\frac{e^{\epsilon}}{g_A}\right)^{\frac{\sigma}{\beta}} R^{\frac{\lambda\sigma}{\beta}} K^{\eta} L^{1-\eta}. \tag{12}$$

TFP is given by:

$$TFP^{*} = \left(\frac{e^{\epsilon}}{g_A}\right)^{\frac{\sigma}{\beta}} R^{\frac{\lambda\sigma}{\beta}}. \tag{13}$$

Taking logs of (13) yields:

$$log(TFP^{*}) = \left(\frac{\sigma}{\beta}\right) log\left(\frac{1}{g_A}\right) + \left(\frac{\lambda\sigma}{\beta}\right) log(R) + \left(\frac{\sigma}{\beta}\right) \epsilon. \tag{14}$$

Our estimation equation for a firm-level panel dataset then, is:

$$log(TFP_{jt}) = \gamma_o + \gamma_1 log(R_{jt}) + \epsilon_{jt}. \tag{15}$$

Our parameter of interest is γ_1, which represents the returns to scale in idea non-rivalry. The

parameter we need for computing the social rate of return to R&D effort is σ. Our model implies that $\sigma = \frac{\gamma_1 \beta}{\lambda}$, so σ can be obtained from our estimates of the knowledge production function above and from (15).

A similar identification challenge to that noted above exists in this context. Firms with a positive unobserved shock to research productivity will seek to employ more researchers leading R_{jt} and ϵ_{jt} to be positive correlated. We use our event event study approach to address this endogeneity problem in a firm level context to obtain a casual estimate of the non-rival ideas returns to scale parameter γ_1. The idea is that firms are differentially exposed to the draft deferral shock depending on the mix of majors that they employ before the policy change. For example, firms who employ many physics majors will be more exposed that those that employ many health majors. As knowledge may diffuse across firms in the longer term we see our design as most appropriate for short run effects. We thus focus on short-run effects - 5 years of less after the policy change.

Measuring Firm-Level Draft Exposure. The NRSTP data contain text-based employer names from 1964 onward. We have standardized this text field and linked firms to Compustat firm identifiers. For each Compustat firm represented within the NRSTP, we compute the share of researchers who were from a state of birth–undergraduate major grouping. We then multiply this share by a shift representing the draft exposure for each state of birth-undergraduate major group across the US. Firm j's overall draft exposure is given by the shift-share:

$$\text{Firm Draft Exposure}_{j,1966} = \sum_{m=1}^{M} s_{jba,1966} \text{Draft Exposure}_{ba,1966}. \tag{16}$$

The share ($s_{sba,1966}$) represents the proportion of firm j's workers from each state of birth-undergraduate major group, ba. The shift (Draft Exposure$_{ba,1966}$) is the draft exposure in grouping ba, discussed above.

Reduced Form Research Expenditure. We again employ a two-step PPML reduced form approach. We first estimate the reduced form effect of the end of graduate draft deferral on firm level research effort (R_{jst}) by PPML using the model:

$$R_{jst} = e^{(\delta_j + \delta_t + \delta_{RF} \text{End} Deferral_t \times Firm Draft Exposure_{j,1966})} \mu_{ist}, \tag{17}$$

where R_{jjt} is research effort by firm j in industry s in year t, δ_j and δ_t are firm and year fixed effects, respectively. End Deferral$_t$ takes a value of one for the first five years after

the draft deferral policy change occurred in 1967. Our coefficient of interest is δ_{RF} which measures the impact of the policy change on research effort. Compustat data do not report firm-level researcher counts, unfortunately. Instead, we use research expenditures to capture the research effort effect. [29]

Reduced Form TFPR. With the research effort estimate in hand, we estimate our second set of reduced form models with revenue and measured Total Factor Productivity (TFPR) as outcome variables.[30]

We estimate the model:

$$log(J_{jst}) = \omega_{RF}\text{Firm Draft Exposure}_s \times Post_t + \tau_t + \tau_j + \epsilon_{jst}, \tag{18}$$

where J_{jst} is the firm-level outcome variable – either TFP or revenue – for firm j in sector s in time period t. τ_t are year fixed effects and τ_j are firm fixed effects. $Post_t$ takes a value of one five years after the draft deferral policy change and zero otherwise. Our parameter of interest is ω_{RF}. We weight firms by the number of scientists they had in 1966 to ensure that the estimates are representative for the average scientist. Standard errors are clustered at the firm level.

Firm Data. To capture the short-term effects of the draft deferral policy change we use Compustat data from 1960 to 1970 for a balanced panel of firms outside of the natural resources industries. We restrict the sample to firms with more than 10 scientists in the NRSTP data in 1966.

Estimation Results. We report the results of our estimations in Table 5. The results in columns (1) and (2) demonstrate that the draft deferral shock led to an increase in firms' R&D spending. These results are consistent with the expectation that the end of graduate school draft deferral would have increased the supply of researchers and firms responded by employing a larger number of them. In columns (3) and (4) we we show that firms facing a

[29] Using this variable may overestimate the researcher effect if an important fraction of firm research expenditure was on capital rather that labor. This approach does not account for any response of laboratory capital utilized in the research process. Excluding capital from the knowledge production process is consistent with Waldinger (2016) who finds that knowledge production only responds to human capital and not physical capital shocks. Furthermore, Lerner et al. (2024) show that 85% of the variation in commercial impacts of research comes from individual researchers rather than universities. Expenditures may be an underestimate of effort if increases in researcher supply reduced researcher wages (Goolsbee 1998). Given the data available in Compustat and the fact that we do not find a wage effect above, we regard the response of R&D expenditure to the end of graduate deferrals as a reasonable proxy for firm level research effort effects.

[30] We measure firm level TFPR as the Solow residual (A_{jst}) using the production function: $Y_{jst} = A_{jst}K_{jst}^{\phi_1}L_{jst}^{\phi_2}$.

larger draft deferral shock experienced growth in sales revenue. These increases in revenues translated into increased measured TFPR, as shown in columns (5) and (6). Our results change little when flexible time trends by headquarter state are included as controls.

Returns to Scale and Idea Non-Rivalry. The returns to scale in idea production reflects how the TFP of goods producers depends on the research input in the ideas sector. We can use our the ratio of the reduced form estimates for TFP and research effort from (18) and (17), respectively, to obtain the returns to scale from non-rival ideas as $\gamma_1 = \frac{\omega_1^{TFP}}{\delta_1^R}$. With γ_1 in hand we can then calculate $\sigma = \frac{\gamma_1 \beta}{\lambda}$. The results in Table 5 imply a returns to scale of 0.23 to 0.39, depending on the specification. Our estimate of the returns to scale from ideas is smaller than what Peters (2022) finds. He uses exogenous variation in the settlement of East Germans into West Germany after World War II and estimates $\gamma_1 = 0.71$ for the manufacturing sector and $\gamma_1 = 0.5$ for the entire economy. This difference could be due to a large influx of population affecting goods producers' productivity through agglomeration, rather than through an ideas channel alone. As we are examining firm level outcomes, business stealing between firms that affected markups and measured TFP would likley attenuate our estimated effect.[31]

Social Rate of Return to R&D. Jones and Williams (1998) show in the simple semi-endogenous growth framework we present above that the social rate of return to R&D, $\tilde{r}(s)$, in steady state is given by:

$$\tilde{r}(s) = \frac{\lambda \sigma g_A}{s} + (1 - \beta) g_A + (g_Y - g_A), \tag{19}$$

where λ is the effort elasticity, σ is the non-rivalry of ideas effect, s is the share of R&D in total output, $1 - \beta$ is the intertemporal elasticity, g_A is the growth rate in ideas, and g_Y is the growth rate of output.

We obtain λ and β from our estimates of the knowledge production function for academic papers reported in Table 2. We obtain σ from our returns to scale estimate from Table 5, in concert with the knowledge production function estimates, as $\sigma = \frac{\gamma_1 \beta}{\lambda}$. We calibrate other parameters using US data from 1960 to 1980. In this period the average annual share of R&D spent on research was $s = 0.024$, the average growth rate of ideas (as measured by the annual growth rate in patenting) was $g_A = 0.023$, and the average annual growth rate of output was $g_Y = 0.037$.

[31] The share of the cross firm effects in Bloom et al. (2013) from business stealing offers one approach to adjust our estimates for business stealing. Doing so yields a similar returns to scale from non-rival ideas of 0.3 to 0.5.

We find a social rate of return to R&D of 31% to 51% using our estimated parameters from the knowledge and goods production functions. Our upper estimate is close to Bloom et al. (2013) who estimate a social return to private R&D of 55%. In terms of putting our rate of return estimate into larger perspective, one comparison would be the return on risky assets which could be an alternative investment option since R&D can certainly be a risky investment. Jordà, et al. (2019) find that the risky rate of return across many counties and time periods is about 7%. Our estimate of the social rate of return to research is many times larger than this estimate.

How has the fact that ideas are getting harder to find changed the social return to R&D? In Table 6 report the results from counterfactual estimates where we compute another version of $\tilde{r}(s)$, but substitute Bloom et al.'s (2020) estimate of $\beta = 3$ for ours. Doing so yields a rate of return of 27% to 48%. While the reductions in research productivity of the more modern era that Bloom et al. report are slightly larger than our, suggesting that ideas have been getting increasingly harder to find, our counterfactual estimates indicate that the social returns to R&D have fallen some over time, yet remain significant in magnitude.

6 Conclusion

We estimate the effect of researchers on innovation and growth using newly available archival data on the scientific workforce of the 1960s. Our research design leverages a unique shock to research labor supply induced by changes in draft-age men's ability to defer their draft eligibility during the most tumultuous period of the Vietnam War. Disparate shocks to researcher supply based on their states of birth and college majors reveals three main findings. First, research effort effects demonstrate little agglomeration or duplication effects for peer-reviewed publications, but significant duplication for patents. Second, intertemporal spillover effects were negative, suggesting that previous findings that ideas are getting harder to find is not a new phenomenon. Third, we find scale economies from non-rival ideas were present, thus leading to significant social returns to researchers' labor supply.

Some limitations of our study suggest areas for future work. First, while we show different effort effects for publications and patents, whether winner-take-most tournament incentives play a significant role in finding a duplication effect for patents is unclear. As duplication and novelty are central to whether R&D activity will generate benefits or not, future work addressing this issue would benefit from examining how the research effort effect depends on the intellectual property rights regime. Second, while policymakers and firms have long claimed shortages of scientific talent, economists have been skeptical that market failures

exist and that supply subsidies in specific fields are warranted (Arrow and Capron 1959, Ryoo and Rosen 2004). Our results that high human capital researchers provide broader externalities to the US economy provide a rationale for policies that seek to ameliorate the market's perceived under-provision of scientific talent.

Finally, our results imply that even though discovering novel innovations seems to be getting more challenging over time, the social returns to R&D remain relatively large. Public R&D has experienced a secular decline in advanced economies over the past 50 years, while productivity stagnates, yet at the same time the returns to R&D remain significant. Unraveling the political economy of this apparent contradiction in social aims remains an important policy question.

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Table 1: Descriptive Statistics, 1960

Sample=	Full	Early-Career Cohort			Mid-Career Cohort			Differences - in - Differences
		1966 Draft Exposure		Differences	1966 Draft Exposure		Differences	
		Above Median	Below Median	(2)-(3)	Above Median	Below Median	(5)-(6)	(4)-(7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcomes:								
Researchers	12.92	3.76	16.38	0.00	6.86	24.82	0.00	0.25
	(37.45)	(7.68)	(41.42)		(10.22)	(59.11)		
Earnings ($1960)	8,498	6,670	7,142	0.00	9,643	10,096	0.02	0.94
	(2.384)	(1,692)	(1,368)		(2,592)	(1,544)		
Publications (1965)	3.62	1.44	3.53	0.00	2.86	6.70	0.00	0.19
	(10.75)	(4.88)	(9.23)		(7.29)	(16.98)		
Patents (1965)	1.28	0.13	1.17	0.00	0.31	3.51	0.00	0.00
	(6.09)	(0.60)	(4.05)		(0.98)	(11.16)		
Undergraduate Education:								
University Human Capital × 100	0.98	-1.17	2.98	0.03	-1.30	3.12	0.01	0.92
	(19.77)	(22.66)	(16.76)		(21.66)	(17.39)		
University Military (1966)	0.04	0.05	0.07	0.25	0.02	0.02	0.53	0.00
	(0.09)	(0.12)	(0.10)		(0.05)	(0.04)		
Employer:								
Type: University	0.26	0.31	0.20	0.00	0.32	0.23	0.00	0.58
	(0.29)	(0.36)	(0.24)		(0.31)	(0.24)		
Type: Private Sector	0.39	0.35	0.48	0.00	0.29	0.43	0.00	0.79
	(0.34)	(0.39)	(0.32)		(0.32)	(0.29)		
Type: Government or Non-Profit	0.35	0.34	0.31	0.47	0.39	0.34	0.04	0.44
	(0.30)	(0.36)	(0.28)		(0.32)	(0.24)		
Military Funded Project (1966)	0.12	0.09	0.13	0.00	0.10	0.17	0.00	0.04
	(0.13)	(0.14)	(0.11)		(0.12)	(0.14)		
Observations	498	250	248		250	248		

Notes: Source: Authors' calculation using NRSTP, USPTO and MAG data as described in the text and appendix at the undergraduate major × state of birth market level using 1960 data. Main entries in columns (1), (2), (3), (5), and (6) are the mean of the variable indicated. Entries in parentheses in columns (1), (2), (3), (5), and (6) are the standard deviation of the variable indicated. Entries in column (4) report the p-value of for the hypothesis test for the differences in means between (2) and (3). Entries in column (7) report the p-value of for the hypothesis test for the differences in means between (5) and (6). Entries in column (4) report the p-value of for the hypothesis test for the difference-in-differences in means between (2) minus (3) compared to (6) minus (5).

Table 2: Researchers and Knowledge Production

Dependent Variable =	Researchers		Log(Earnings)		Publications		Patents	
Model=	PPML	PPML	OLS	OLS	PPML	PPML	PPML	PPML
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Early Career$_c$ × Draft Exposure$_{m,1966}$ × End Deferral$_t$	41.64	42.01	-0.18	-0.22	14.92	13.00	1.04	1.91
	(4.11)	(4.00)	(0.11)	(0.11)	(2.05)	(2.42)	(3.95)	(4.31)
Observations	5,070	5,070	4,214	4,214	5,070	5,070	2,154	2,154
Research Effort Effect: λ					1.06	0.90	0.10	0.19
Intertemporal Effect: β					1.34	1.36	2.36	2.38
Additional Controls:								
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Labor Market Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Military Funding $_{ck,1966}$ × Year FE		Yes		Yes		Yes		Yes
Military Employment $_{ck,1966}$ × Year FE		Yes		Yes		Yes		Yes

Notes: Source: Authors' calculation using NRSTP, USPTO and MAG data as described in the text and appendix. Column (1) reports estimates of (3) estimated by PPML. Column (3) reports the log transformed analog to equation (3) estimated by OLS. Column (5) and (7) reports estimates of (3) estimated by PPML. Columns (2), (4), (6) and (8) also control for *Military Funding*$_{cm,1966}$ and *Military Employment*$_{cm,1966}$ for each cohort-market × year fixed effects. Patents and Publications are annual market level aggregated measured 5 years after the event. *Draft Exposure*$_{m,1966}$ is the fraction of males less than age 26 in the early career cohort in 1966 in market *m* as given in equation (1). *End Deferral*$_t$ takes a value of one for the years 1968 and 1970 (inclusive) and takes a value of zero otherwise. A labor market is defined at the undergraduate major × state of birth level. The main entries are the mean of the marginal effect with standard errors in parentheses. Standard errors are clustered at the market level.

Table 3: Researchers and Knowledge Production – Alternative Levels of Aggregation

Dependent Variable =	Researchers	Log(Earning)	Publications	Patents
Model =	PPML	OLS	PPML	PPML
	(1)	(2)	(3)	(4)
Panel A: State of Birth × Undergraduate Major				
Early Career$_c$ × Draft Exposure$_{m,1966}$ × End	41.64	-0.18	14.92	1.04
Deferral$_t$	(4.01)	(0.11)	(2.84)	(3.38)
Observations	5,070	4,214	4,070	2,154
Research Effort Effect: λ			1.06	0.10
Intertemporal Effect: β			1.34	2.37
Panel B: Census Region of Birth × Undergraduate Major				
Early Career$_c$ × Draft Exposure$_{m,1966}$ × End	165.31	-0.05	79.14	-13.01
Deferral$_t$	(40.34)	(0.27)	(20.40)	(17.92)
Observations	1,686	1,536	1,686	970
Research Effort Effect: λ			1.42	-0.44
Intertemporal Effect: β			1.22	1.63
Panel C: Census Division of Birth × Undergraduate Major				
Early Career$_c$ × Draft Exposure$_{m,1966}$ × End	466.06	0.21	232.08	-34.97
Deferral$_t$	(141.11)	(0.43)	(64.53)	(31.60)
Observations	802	768	802	574
Research Effort Effect: λ			1.48	-0.52
Intertemporal Effect: β			1.18	1.54
Panel D: National Undergraduate Major				
Early Career$_c$ × Draft Exposure$_{m,1966}$ × End	13786.74	4.46	4681.65	-30.28
Deferral$_t$	(2752.01)	(2.89)	(1365.60)	(674.26)
Observations	216	214	216	192
Research Effort Effect: λ			1.01	-0.02
Intertemporal Effect: β			1.14	1.41
Additional Controls:				
Year Fixed Effects	Yes	Yes	Yes	Yes
Labor Market Fixed Effects	Yes	Yes	Yes	Yes

Notes: Source: Authors' calculation using NRSTP, USPTO and MAG data as described in the text and appendix. Column (1) reports estimates of (3) estimated by PPML. Column (2) reports the log transformed analog to equation (3) estimated by OLS. Column (3) and (4) reports estimates of (3) estimated by PPML. Patents and Publications are annual market level aggregates measured 5 years after the event. *Draft Exposure*$_{m,1966}$ is the fraction of males less than age 26 in the early career cohort in 1966 in market *m* as given in equation (1). *End Deferral*$_t$ takes a value of one for the years 1968 and 1970 (inclusive) and takes a value of zero otherwise. A labor market is defined at the undergraduate major × state of birth level in panel A, undergraduate major × census region of birth level in panel B, undergraduate major × census division of birth level in panel C, and undergraduate major level in panel D. The main entries are the mean of the marginal effect with standard errors in parentheses. Standard errors are clustered at the market level.

Table 4: Researchers and Knowledge Production - Short and Long Term Effects

Dependent Variable =	Publications		Patents	
Model =	PPML	PPML	PPML	PPML
	(1)	(2)	(3)	(4)
Panel A: Regressions				
Early Career$_c$ × Draft Exposure$_{m,1966}$ × Short Term$_t$	6.09	5.93	6.95	7.80
	(1.03)	(1.10)	(2.25)	(2.29)
Early Career$_c$ × Draft Exposure$_{m,1966}$ × Long Term$_t$	9.53	9.09	9.50	10.52
	(1.51)	(1.56)	(3.18)	(3.28)
Observations	18,730	18,730	7,496	7,496
Panel B: Calibrations				
Permanent Shock: Short-Run β	1.52	1.93	2.10	2.54
Permanent Shock: Long-Run β	0.98	1.30	1.25	1.58
Steady State: Short-Run β	1.58	1.60	2.18	2.09
Steady State: Long-Run β	1.33	1.34	1.67	1.64
Additional Controls:				
Year Fixed Effects	Yes	Yes	Yes	Yes
Labor Market Fixed Effects	Yes	Yes	Yes	Yes
Military Funding$_{ck,1966}$ × Year FE	No	Yes	No	Yes
Military Employment$_{ck,1966}$ × Year FE	No	Yes	No	Yes

Notes: Source: Authors' calculation using NRSTP, USPTO and MAG data as described in the text and appendix. Each column reports the results from estimating one version of equation (8) by PPML. Patents and Publications are annual market level aggregated measured 5 years after the event. *Draft Exposure*$_{m,1966}$ is the fraction of males less than age 26 in the early career cohort in 1966 in market *m* as expressed in equation (1). *Short Term*$_t$ takes a value of one for the years 1968 to 1972 (inclusive) and takes a value of zero otherwise. *Long Term*$_t$ takes a value of one after 1972 and takes a value of zero otherwise. The main entries are the mean of the marginal effect with standard errors in parentheses. Standard errors are clustered at the market level. The calibration approach for panel B is described on page 22 of the main text.

Table 5: Firm R&D Spending, Sales, and TFP Effects

Dependent Variable =	R&D Expenditure		Log (Sales)		Log(TFPR)	
Model=	PPML	PPML	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Draft Exposure$_{j,1966}$ × End Deferral$_t$	1363.12	1077.76	13.41	15.28	5.49	7.28
	(372.96)	(394.85)	(4.05)	(5.373)	(2.34)	(2.84)
Observations	11,315	11,315	34,705	34,705	34,705	34,705
Ideas Returns to Scale Effect: γ					0.23	0.39
Additional Controls:						
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State$_j$ × Year FE	No	Yes	No	Yes	No	Yes

Notes: Source: Authors' calculation using NRSTP and Compustat data as described in the text and appendix. Columns (1) and (2) reports the results from estimating equation (16) by PPML. The main entries columns (1) and (2) are the mean of the marginal effect and standard error. Columns (3)-(6) reports the results from estimating equation (18) by OLS. The main entries in columns (3) to (6) are coefficient estimates with standard errors in parentheses. The models in columns (1), (3) and (5) control for year and firm fixed effects. The models in columns (2), (4) and (6) also include controls for firm headquarters state times year fixed effects. Log(TFPR) is estimated from estimating: $Log(Sales_{jt})=Log(TFP_{jt})+\varphi_1 Log(Employee\ Expense_{jt})+ \varphi_2 Log(Fixed\ Assets_{jt}) + \lambda_j + \lambda_t + \varepsilon_{jt}$. Standard errors are clustered by firm. All financial variables are in nominal $.

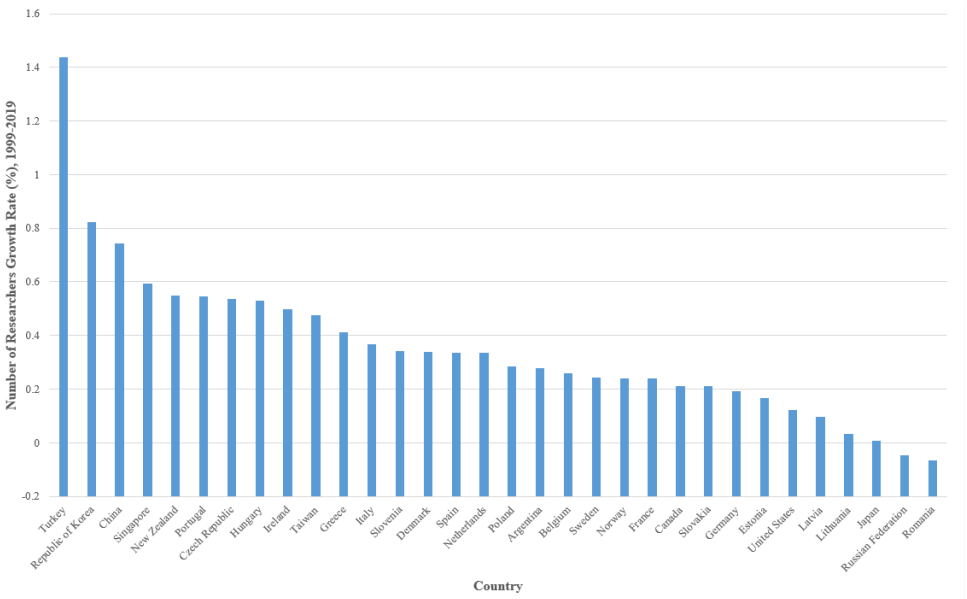
Table 6: Non-Rival Ideas and Social Returns to R&D

Ideas Returns to Scale:	$\gamma = 0.23$	$\gamma = 0.39$
Non-Rivalry (σ)	0.30	0.50
Social Rate of Return to R&D – Estimated β	31%	51%
Social Rate of Return to R&D – Bloom et al. (2020) β	27%	48%

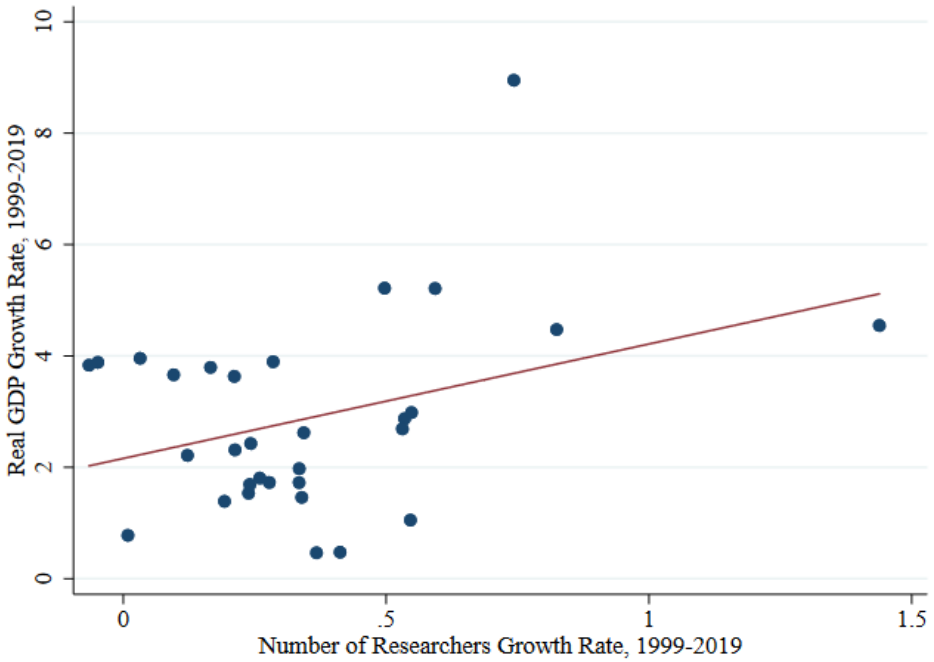
Notes: Source: Authors' calculation using knowledge production estimates from Table 2 column (5). Calibrations are for the Jones (2022) social welfare model for the case where α=1 and the labor supply elasticity = 0.2. The two estimates of the elasticity of the returns to scale from the estimates in Table 6 are indicated in the column headings. All other parameters take the values employed by Jones (2022).

Figure 1: Cross-Country Research Effort

Panel A: Researcher Growth Rate, By Country

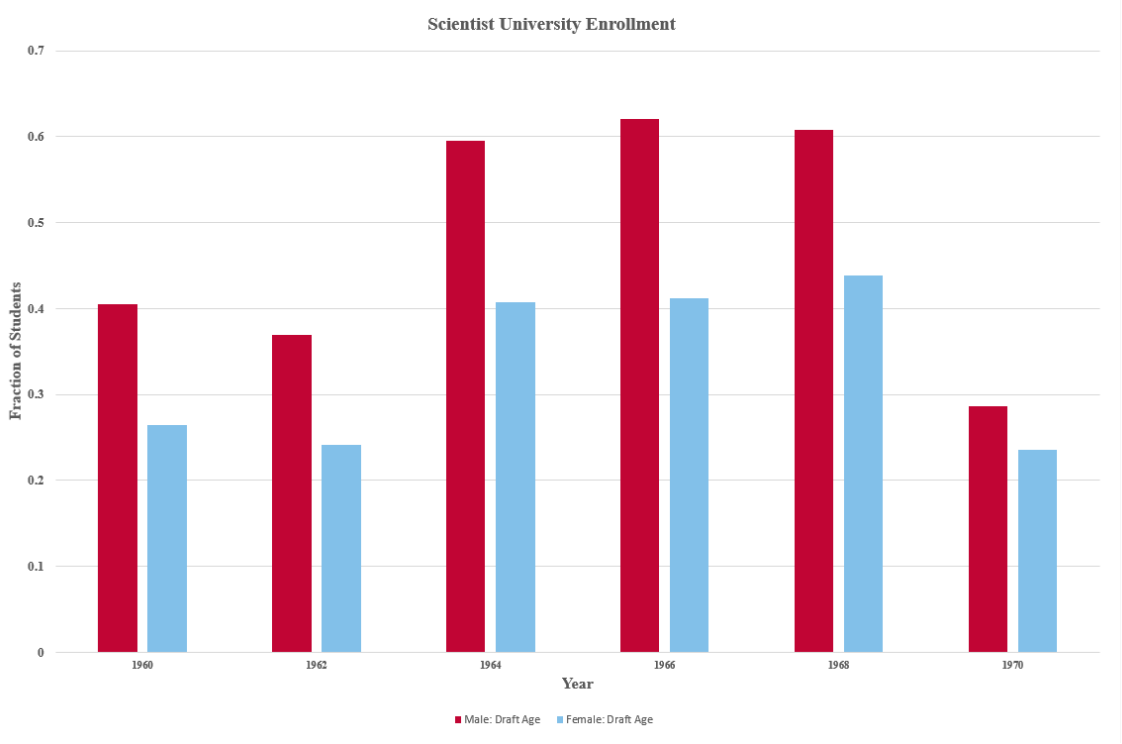


Panel B: Researcher Growth and Real GDP Growth



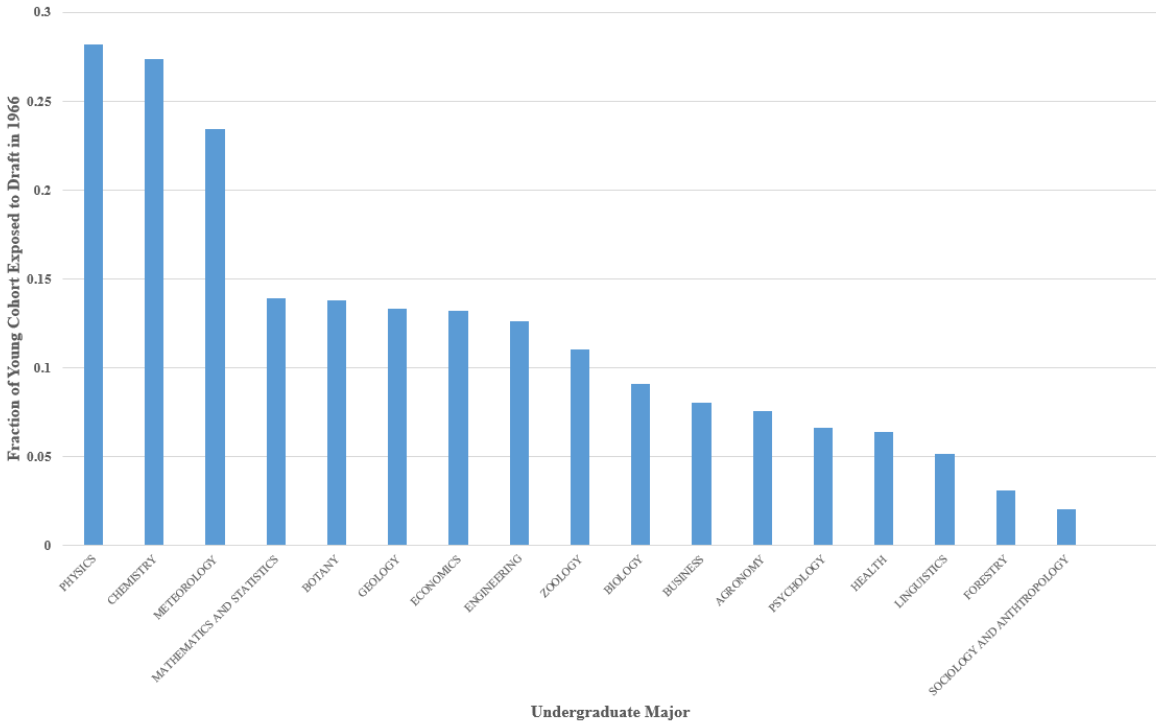
Notes: Source: Authors' calculations using OECD and World Bank data from 1999 to 2019.

Figure 2: University Enrollment Among Surveyed Researchers



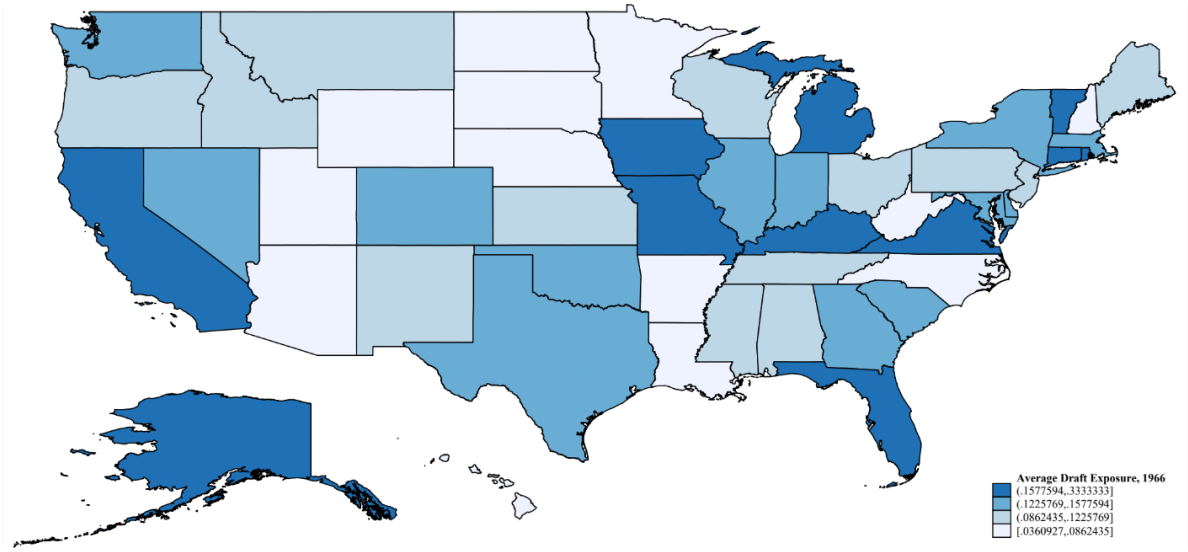
Notes: Source Authors' Calculations with NRSTP data.

Figure 3: Market Draft Exposure in 1966, by Undergraduate Major



Notes: Source Authors' Calculations with NRSTP data.

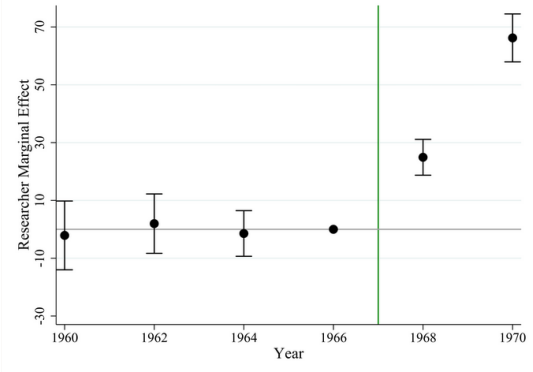
Figure 4: Market Draft Exposure in 1966, by State of Birth



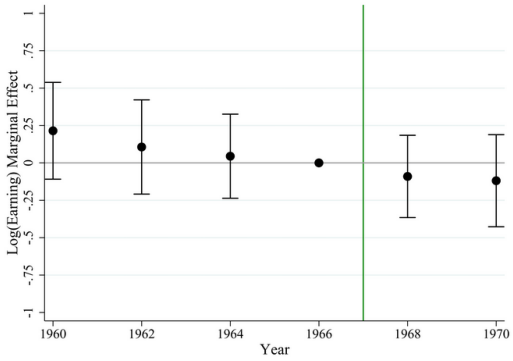
Notes: Source Authors' Calculations with NRSTP data.

Figure 5: Dynamic Labor Market and Knowledge Production Effects

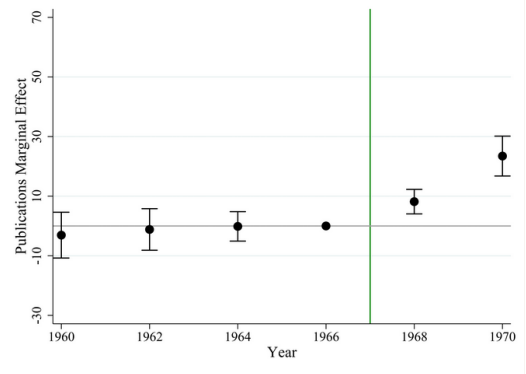
Panel A: Researchers



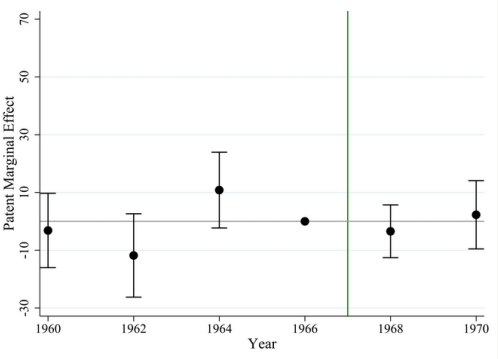
Panel B: Log(Labor Income)



Panel C: Publications



Panel D: Patents



Notes: Source: Authors' calculation using NRSTP, USPTO and MAG data as described in the text and appendix. Panel A reports estimates of (7) estimated by PPML. Panel B reports the log transformed analog to equation (7) estimated by OLS. Panel C and D reports estimates of (7) estimated by PPML. Patents and Publications are annual market level aggregated measured 5 years after the event. *Draft Exposure*$_{m,1966}$ is the fraction of males less than age 26 in the early career cohort in 1966 in market *m* as given in equation (1). A labor market is defined at the undergraduate major × state of birth level. The main entries are the mean of the marginal effect with standard errors in parentheses. Standard errors are clustered at the market level.