Can Displaced Labor Be Retrained? Evidence from Quasi-Random Assignment to Trade Adjustment Assistance*

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Abstract

The extent to which workers adjust to labor market disruptions in light of increasing pressure from trade and automation commands widespread concern. However, surprisingly little is known about efforts that deliberately target the adjustment process. This project studies 20 years of worker-level earnings and re-employment responses to Trade Adjustment Assistance (TAA)—the United States' largest and longest standing public incentive program for retraining. I estimate causal effects from the quasi-random assignment of TAA cases to investigators of varying approval leniencies. Using employer-employee matched Census data on 300,000 displaced workers, I find large initial returns to TAA. Ten years out, TAA-trained workers have $50,000 higher cumulative earnings, driven by both higher incomes and greater labor force participation. Yet *annual* returns fully depreciate after ten years. TAA appears to have no effect on formal education, and decaying returns are concentrated in states with lower training durations. Combined with evidence that effects are driven by training rather than unemployment insurance transfers, TAA appears to augment transient rather than permanent components of human capital. Returns are further concentrated in the most disrupted regions, where workers are more likely to switch industries and move to labor markets with better opportunities in response to training—consistent with adjustment frictions.

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1 Introduction

Between 2000 and 2016 the US shed approximately 6 million manufacturing jobs, resulting in the lowest level of manufacturing employment since the onset of World War II (BLS, 2017).[1] Strikingly, this decline contrasted with record revenue growth in both manufacturing and non-manufacturing industries over the same period (see Figure 1). In light of increasing pressure from trade and automation, are today's workers able to adjust to labor market disruptions as they have in the past?

While economists have generally considered worker adjustment to shocks such as trade to be relatively frictionless, the unusual speed of these recent structural changes has prompted a fresh look at whether growing industries can absorb and offset declining earnings among displaced workers. Standard Ricardian trade theories predict that relatively higher- and lower-skilled economies stand to "gain from trade" if skill-intensive labor markets specialize in high value-added activities (like computer software) and trade for lower value-added goods and services (such as textiles). However, these models also conventionally assume that workers facing short-run job displacement from trade either adjust instantaneously to newly expanding sectors (i.e. labor is perfectly mobile), or are compensated by redistributive government transfers that preserve trade's Pareto-improving qualities.[2]

Though many studies have shown that removing trade barriers can increase growth and consumer welfare through specialization, lower goods prices, and higher variety,[3] less attention has been given to adverse labor outcomes because imports from low-wage countries had been relatively inconsequential until the 1990s (Autor et al., 2016). More recently however, influential papers in empirical trade and labor economics have documented that displaced workers may remain persistently underemployed and underpaid (with respect to prior earnings) years beyond their initial job separation (Bartik A., 2017; Lachowksa et al., 2017; Pierce and Schott, 2016; Flaaen et al., 2016; Autor et al., 2014, 2013; Autor and Dorn, 2013; Harrison and McMillan, 2011).[4] As jobs and tasks become increasingly outsourceable and automated, there is also growing concern about how future generations of US workers will sustain wage growth in a rapidly transforming innovation economy. For example, 1.3 million truck drivers will likely compete with the emergence of self-driving vehicle technology by 2026 (CEA, 2016). Such issues were

[1] Pierce and Schott (2016) highlight that the recent manufacturing decline contrasts with a stable employment trend which had varied around 18 million jobs from 1965 until the accession of China to the World Trade Organization (WTO) in 2001.

[2] Economists have relied on the notion of "Kaldor-Hicks Efficiency" (Kaldor, 1939; Hicks, 1939) to relax strong Pareto assumptions which preclude "losers" from trade. Instead, an allocation is Kaldor-Hicks efficient if losing factors of production can *hypothetically* be compensated by winning factors. Building on the work of Dixit and Norman (1986), Feenstra and Lewis (1994) show that redistributive compensation from levying taxes on winning factors are more likely to result in a Pareto-improving allocation if combined with mobility subsidies to overcome adjustment frictions.

[3] Recent work by Fajgelbaum and Khandelwal (2016) further suggests low-income workers have the *most* to gain from trade, as they bear a disproportionate incidence of lower consumer prices due to a higher propensity to consume tradable goods.

[4] Feenstra et al. (2017) and Feenstra and Sasahara (2017) find that manufacturing job losses are offset by service job growth, however these papers do not address distributional consequences which can persist despite aggregate gains.

especially salient during the 2016 US Presidential election, which featured protectionist backlash against trade on both sides of the political aisle.[5]

Yet in spite of widespread concern and a growing literature on adjustment frictions, surprisingly little is known about whether the US's largest and longest standing incentive program for retraining displaced workers—the Department of Labor's 1974 Trade Adjustment Assistance (TAA) program—is a necessary, effective, or efficient means to accelerate adjustment. The main contribution of this paper is to provide large-sample empirical estimates of the causal effects of retraining trade-impacted workers on labor market outcomes. Credible parameter estimates of these effects have historically been complicated by two factors: (1) A lack of detailed worker-level data to track TAA participants before and after displacement events across employers; (2) Confounding factors correlated with qualifying for TAA, particularly pre-determined skills and trends associated with tradable-good production and training take-up—selection biases which preclude reliable estimates of the program's effects. In this paper, I employ a quasi-experimental research design that builds on the rapidly maturing examiner (judge randomization) methodological literature to circumvent these endogeneity concerns. Applying this strategy in a "big data" setting, I estimate the causal effects of TAA benefits on displaced worker earnings, employment, education, and mobility outcomes from 1990 to 2011.[6]

I first assemble a new dataset combining restricted-use administrative data from the US Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) dataset with the universe of TAA winning and losing petitions (applications) attained through Freedom of Information Act requests at the US Department of Labor (DOL). This unique merger allows me to track a sample of approximately 300,000 displaced workers as they move in and out of unemployment status and across employers of diverse industries and regions, both before and after their initial job separation. Critically, DOL petition data contain the unique names of investigators responsible for determining whether workers qualify for TAA. These investigators are tasked with subjectively determining whether applicants were laid off by companies whose decline in production (sales) was due to increased imports or offshoring—an adjudication of the firm's trade exposure or "tradability" status.

I show that this institutional feature of TAA effectively assigns two otherwise identical worker cohorts (displaced from the same industry), different TAA approval probabilities based on whether their case is directed to a more *lenient* versus *strict* investigator. If assigned quasi-randomly to more lenient investigators, displaced workers have a higher likelihood of receiving TAA benefits. These include

[5] Autor et al. (2016) study the effects of rising trade exposure on political polarization, and find that exposure to trade with China decreased the number of moderate congressional representatives in office.

[6] Recent examples of such examiner designs include Dobbie and Song (2015), who use the random assignment of bankruptcy judges to establish the causal effects of consumer bankruptcy insurance on debtor outcomes; Autor et al. (2015), who study the welfare impact of disability benefits in Norway; and Mueller-Smith (2015), who studies the impacts of incarceration. The original idea to exploit judge randomization as a causal design can be attributed to Kling (2006).

up to $10,000/year for two years of training and on average $15,000/year of extended unemployment insurance (UI) while training. I leverage this assignment as an exogenous source of variation in training take-up to identify the causal effects of TAA in a two stage least squares (2SLS) setting—mapping out differential and dynamic earnings responses using investigator leniency as an instrumental variable.[7]

I find evidence of large initial returns to TAA. Workers inferred to take up benefits forego roughly $10,000 in income while training, yet ten years later have approximately $50,000 higher cumulative earnings relative to all-else-equal workers that do not retrain. I estimate that 40% of these returns are driven by higher wages—a sizable share which suggests that TAA-trained workers are not only compensated through greater labor force participation or higher priority in job queues. Rather, TAA workers also appear to be paid a premium for their newly acquired human capital. But these large relative gains also *decay* over time. In fact, annual incomes among TAA and non-TAA workers fully converge after ten years. In conjunction with two additional pieces of evidence—that TAA has no effect on formal education, and diminishing returns are restricted to states with low training durations—I attribute this depreciation to short-run demanded skills becoming obsolete (consistent with rapid skill-biased technological change or an overall declining labor share).[8] Indeed, 62% of TAA training programs confer vocational degrees with shorter program lengths than typical community college or 4-year college degrees which have been shown to have durable earnings returns (Card (2001); Kane and Rouse (1995)).

While these results provide strong evidence that overall earnings returns to TAA are largely positive, government intervention in the adjustment process is only warranted economically in the presence of market failures or if redistribution is socially desirable.[9] While human capital theory suggests that wage differentials across occupations should provide ample incentives for workers and firms to privately undertake skill upgrading (Becker, 1964), laid off machine operators in Detroit's automobile sector may face a variety of frictions that prevent them from acquiring productive employment in expanding sectors such as robotics occupations in Pittsburgh's burgeoning 3D-printing industry. Among several possible barriers to adjustment, these frictions may be *spatial* or *industrial*—high mobility costs across labor markets and industries stagnate worker wages in trade-afflicted labor markets (Bartik A., 2017; Yagan, 2014; Kline and Moretti, 2013; Blanchard and Katz, 1992); *informational*—search costs result in job mismatch after displacement (Moretensen and Pissarides, 1994); *financial*—liquidity constraints preclude the necessary investments in retraining and education required for work in the modern

[7]While this design is well-equipped to estimate an important adjustment parameter with respect to training, the TAA treatment effect (estimated here using an intent-to-treat framework) should be thought of as just one input into a richer trade model characterizing the full efficiency and distributional consequences of trade liberalization.

[8]For a discussion of the potential determinants of the declining labor share in the United States, see Autor et al. (2017).

[9]There are arguably other less distortive ways to achieve the latter redistributive goal. See for example recent work by Lyon and Waugh (2017), who consider optimal progressive taxation schemes to redistribute the gains from trade.

economy (Lochner and Monge-Naranjo, 2011); or *behavioral*—workers may be "present-biased" or make forecasting errors about the future viability of their local industries (Augenblick et al., 2015).

To unpack which frictions underlie the main effects (if any), I conduct a variety of empirical tests exploiting the rich heterogeneity of the administrative data to identify potential mechanisms. I find strongest support for spatial and industrial adjustment frictions. Merging county-level Bureau of Labor Statistics (BLS) unemployment rate data to the LEHD panel, I define "high" and "low" shock severity regions based on whether the county in which a TAA-qualified worker was displaced was above or below median unemployment in their quarter of separation. I find that workers in highly disrupted regions are more likely to switch both industries *and* commuting zones (a now widely used geographic measure of local labor markets) in response to training. Workers are approximately 30 percentage points more likely to move commuting zones and 50 percentage points more likely to switch industries (at the 2-digit North American Industrial Classification System (NAICS) level), with respect to the location and industry of their pre-layoff employer. While not as well identified due to potential selection concerns (but nonetheless supported by common pre-trends), I also present suggestive evidence that positive earnings returns among "movers" drive the overall effects relative to "stayers".[10] Lastly, I find no evidence that TAA subsidies lead to deferred job search, which suggests that effects are more likely due to the training itself rather than relieving liquidity or search constraints.

While the paper's identification strategy provides robust evidence of positive earnings returns and higher mobility associated with TAA, this does not inform us about the cost-effectiveness of the program. Toward this second end, I compare the ten year stream of estimated TAA differential earnings returns as benefits, with average TAA expenditures on training, extended UI, and foregone earnings while training, as costs. I estimate an internal rate of return (IRR) to TAA of between 0.0% and 9.1%, which I interpret as a lower bound for two reasons. First, earnings returns are calculated from an intent-to-treat (ITT) estimator which likely understates benefits due to imperfect compliance with the treatment (i.e. partial take-up of TAA attenuates earnings estimates toward zero and understate the treatment-on-treated (TOT) effect of interest). Second, TAA may induce worker substitution away from other costly social insurance programs such as disability insurance (DI), which would further understate program costs.[11]

One important caveat however, is that positive earnings returns are estimated from a local average treatment effect (LATE) that does not capture the fact that firms may be incentivized to lay off *additional* workers after learning of their TAA eligibility status. Nevertheless, back-of-the-envelope calculations show that these are unlikely to outweigh potentially much larger downward pressures.

[10] As is discussed further below, whether a worker switches or stays in their initial commuting zone or industry is defined by their *ex-post* mobility decisions, which may reflect a self-selected sample that differs along a number of unobservables.

[11] Autor et al. (2014) find that DI is in fact the predominant margin through which workers adjust to trade shocks.

Overall, this paper provides new quasi-experimental evidence that earnings returns from trade-adjustment targeting via retraining may be larger and more effective than previously thought. More work is needed however, to fully understand the extent to which such targeting alleviates aggregate worker adjustment barriers. Despite some noteworthy inefficiencies, TAA may serve as an important tag for redistributing the growth from trade. However, whether these results can be extrapolated to future types of trade and automation pressures remains an open question.

1.1 Related Literature

While this paper is one of the first to empirically estimate the effects of TAA on worker outcomes, the study builds on a long history of scholarship in labor economics, trade, and public economics. Particularly, this analysis is positioned at the intersection of three distinct literatures:

(1) The impact of worker displacement and trade on inequality.

(2) The effects of job training on earnings and human capital accumulation.

(3) The role of local labor market and occupational mobility frictions in earnings growth.

Starting with displacement and inequality, this paper most directly follows the work of Autor et al. (2016), who study long run earnings adjustment patterns of workers exposed to Chinese import competition using administrative Social Security Administration (SSA) data. The authors find long-run negative effects from trade, and show that trade-affected workers take up disability insurance (DI) rather than trade adjustment assistance to insure themselves against these shocks.[12] Recently, other scholars have also studied displacement and inequality through the lenses of trade (Pierce and Schott, 2016; Ebenstein et al., 2014; Autor et al., 2013; Amiti and Davis, 2011; Harrison and McMillan, 2011; Kletzer, 2004), technology and automation (Autor et al., 2013; Acemoglu and Autor, 2011; Goldin and Katz, 2009), business cycle fluctuations (Lachowksa et al. (2017), Von Wachter et al. (2009)), and reasons for separation (Flaaen et al., 2016). One additional catalyst for renewed interest in this topic has been greater availability of administrative datasets which facilitate more precise estimates of heterogeneous responses to different types of separation events.

Work on displacement has also been linked to a longer tradition of scholarship on job training, which has been the source of several methodological advancements in labor economics and debate surrounding the econometrics of program evaluation. Early literature focused on either model-based econometric selection correction methods (Heckman, 1976) or difference-in-differences estimators (Ashenfelter (1978)) to address a number of endogeneity concerns, and found little to no effects of job training on

[12] One can compare these persistent adverse effects form import competition to the relative returns to retraining through TAA. Importantly however, Autor et al. (2016) consider the joint effect on workers who may see reduced earnings or hours on the intensive margin, as well as workers who are displaced (the unit of analysis in this paper). These sample differences thus present important caveats when making direct estimate comparisons.

worker outcomes. Ashenfelter and Card (1985) however, showed that difference-in-differences estimates were sensitive to the control groups used, while LaLonde (1986) demonstrated that experimental results did not match non-randomized econometric studies. This was followed by rapid advancement in the development of matching estimators and propensity score methods (Heckman et al., 1996, 1997), and continued debate over how to resolve the critique of nonexperimental approaches to estimation.[13]

In their 1999 handbook chapter, Heckman, LaLonde, and Smith (1999) bridge the debate by calling for better data and experimental evidence when possible. Subsequently, in a meta-analysis of 97 job training program evaluations from 1995 to 2007 that more frequently employ experimental variation and higher quality data, Card et al. (2010) follow up on prior estimates with a similarly titled handbook chapter, and conclude that "...training programmes are associated with positive medium-term impacts, although in the short term they often appear ineffective." When converted to standardized effect sizes, this paper's main employment results fall near the middle of those effect sizes classified as "significantly positive" in the Card et al. (2010) study.

While TAA is a sizable training program, there is a surprising dearth of evidence on its empirical effects on workers.[14] One notable exception is a recent study by Mathematica Policy Research authors Schochet et al. (2012), who evaluate the effects of TAA by comparing program enrollees with a propensity-score matched sample of UI claimants that do not take up TAA. The authors find a mostly negative result, concluding that "...impacts of TAA on engagement in any productive activity were small." However, they are also very careful to note two important caveats in interpreting their effects. First, the sample frame used in their study included manufacturing workers who were laid off just before a large economic downturn—sampled between 2005 to 2006. Since TAA trainees enroll for up to 2 to 3 years and the study only tracked workers for 4 years after enrollment, TAA workers would have entered a more dismal labor market relative to matched UI claimants who may have been reemployed during a more robust economy after exhausting short-run UI benefits.[15] Second, and consistent with conclusions from the propensity score literature which calls for a large number of observables for internal validity (Todd (2010)), the authors recognize that unobserved heterogeneity may result in differently selected samples across the two groups. By contrast, this study leverages a quasi-experimental design and tracks workers over 20 years using a wide range of administrative and demographic variables.

The third literature this paper builds on relates to imperfect spatial and industrial mobility.

[13] Dehejia and Wahba (1999) first indicated that LaLonde's critique could be addressed using propensity score methods, but later work by Smith and Todd (2001) showed the limitations of this approach.

[14] Notable exceptions include Feenstra and Lewis (1994), who provide a strong theoretical foundation for considering the role of adjustment assistance in the context of imperfect mobility. Monarch et al. (2017) and Kondo (2013) also use TAA-approved plants as a lever to study the effects of offshoring on firms and displacement multipliers respectively.

[15] Standard UI benefits typically last for 26 weeks, however during the Great Recession both generosity and duration were greatly expanded.

Pioneering work by Blanchard and Katz (1992) highlighted the empirical importance of worker mobility as an equilibrating factor across differently shocked labor markets, and has been followed more recently by studies illuminating the need for careful estimates of how mobility constraints affect worker outcomes. These questions have been studied in the context of migratory insurance during recessions (Yagan (2014)), responses to trade shocks ((Dix-Carneiro and Kovak, 2017; Bartik A., 2017; Kovak et al., 2017)), and more broadly in the context of place-based policies and taxation (Serrato-Suárez and Zidar (2016), Kline and Moretti (2014, 2013)). In these and other papers, relaxing the benchmark of a perfectly elastic labor supply across regions, sectors, and occupations, generate all types of frictions which are only now beginning to be tested empirically. In this paper, I contribute direct causal estimates of how training programs affect worker mobility across local labor markets and industries.

The rest of the paper proceeds as follows. Section 2 provides institutional background on Trade Adjustment Assistance, worker eligibility for benefits, and the composition of training programs taken up. In Section 3, I discuss the main administrative data sources used, matching procedures and sample restrictions. Section 4 outlines the investigator leniency identification strategy and estimating equations. Section 5 presents the main results on worker-level outcomes, and heterogeneous mobility and training intensity effects. I contextualize these results in a cost-benefit analysis in Section 6, presenting the internal rate of return of TAA and sensitivity to assumptions. Section 7 concludes.

2 Trade Adjustment Assistance

> *When considerations of national policy make it desirable to avoid higher tariffs, those injured by that competition should not be required to bear the full brunt of the impact. Rather, the burden of economic adjustment should be borne in part by the Federal Government... But the accent is on "adjustment" more than "assistance". Through trade adjustment prompt and effective help can be given to those suffering genuine hardship in adjusting to import competition, moving men and resources of uneconomic production into efficient production and competitive positions...*

—John F. Kennedy, Congressional address on the 1962 Trade Expansion Act (originally cited by Kondo (2013))[16]

2.1 Institutional Background

US Trade Adjustment Assistance (TAA) is a federal transfer program established under the 1962 Trade Expansion Act which provides assistance to workers "who lose their jobs or whose hours of work and

[16]Institutional details in this section build on those presented in Monarch et al. (2017) and Kondo (2013), and are amended based on secondary sources and interviews with Office of Trade Adjustment Assistance officials.

wages are reduced as a result of increased imports or shifts in production out of the United States."[17] The program was established during the Kennedy Round of the General Agreement on Tariffs and Trade (GATT), and sought to couple unprecedented trade liberalization (part of a more widespread post-war trade integration effort) with adjustment insurance for adversely affected US workers.

The current program is governed by the 1974 Trade Act, and was amended several times throughout the 1980s altering the generosity of its benefits. In 1993, TAA was expanded to add additional coverage for workers directly affected by the North American Free Trade Agreement (NAFTA-TAA), however this provision was subsequently repealed in the Trade Act of 2002. Since 2002 however, TAA benefits have remained relatively stable in expenditure terms. In fiscal year 2010, nearly $1 billion in annual cash transfers were appropriated to subsidize an estimated 230,000 qualified workers to enroll in retraining programs after trade-related layoffs.[18] While TAA contains several program components, its primary benefit is coverage of up to $10,000/recipient for every year a qualified worker is retraining, up to a statutory maximum of three years. Coverage includes up to two years for "basic" retraining, and an additional year for "remedial" training (if deemed necessary) or "completion" training for workers who are close to completing a credentialed curriculum but have exhausted basic benefits.

Recipients are also entitled to expanded unemployment insurance (UI) benefits while training (called "Trade Readjustment Allowances" (TRA)), *conditional* on providing regular proof of training enrollment. This conditional incentive design is distinct from standard UI, in that it intends to overcome moral hazard concerns associated with search disincentives, imposing at least minimal "search effort" through job training.[19] Workers are eligible for extended UI for up to three years, including the standard initial 26-week UI duration which is most common across US states. Median receipts on extended UI roughly equaled $15,000/recipient-year from 2001 to 2016, slightly more than the $10,000/recipient-year offered for training.[20] Lastly, TAA covers minimal job search and relocation costs, yet these comprise less than 0.1% of total spending, and are hence given little attention throughout the analysis.[21]

TAA funds are allocated via state transfers to Cooperating State Agencies (CSAs) from the Federal Unemployment Benefits and Allowances (FUBA) account. Once workers are approved for TAA, state career centers (e.g. American Job Centers, One-Stop Career Centers) guide workers to potential training program matches based on prior experience. However, workers ultimately have choice over where to train. Once they have demonstrated proof of enrollment, both training subsidies and regular TRA

[17] Department of Labor, Employment and Training Administration. https://www.doleta.gov/tradeact/taa_wdp.cfm

[18] Department of Labor, Employment and Training Administration. https://www.doleta.gov/tradeact/factsheet.cf.

[19] Workers near retirement age however, may not find time and effort costs of training to be a high trade-off for extended UI.

[20] The extent of this support varies formulaicly by each state's initial UI generosity. Data used for this calculation comes from the Trade Act Participant Report (TAPR) from 2001 to 2016. See Appendix B.4 for more details.

[21] TAA has also been experimenting with wage insurance to bridge the income gap for older workers, however this program (know as Alternative or Reemployment TAA) is beyond the scope of the current project.

payments are administered through local state career centers, where workers recoup paychecks.

2.2 Worker Eligibility

To receive TAA benefits, workers (or their surrogates) must file petitions at the Department of Labor within one year of their trade-related separation from a given employer. Filers may include groups of three or more workers (38% of total filers), companies filing on behalf of laid off workers (36%), labor unions (13%), or state career centers (13%).[22] From 1974 to 2016, the Department of Labor received petitions covering an estimated 8.2 million workers, of which approximately 63% were approved for TAA (see Figure 2). Petitions were filed on behalf of workers from more than 40,000 unique plants, and the number of approved applicants began to outstrip denied applicants after the passage of NAFTA.[23]

Once a petition is filed at the Department of Labor (DOL), Office of Trade Adjustment Assistance supervisors ("certifying officers") assign petitions to case investigators working from the Washington D.C. headquarters.[24] (This assignment process is described in greater detail in the identification strategy in Section 4.) These investigators are tasked with subjectively determining whether applicants were laid off by companies whose decline in production (sales) was due to increased imports or outsourcing, and have subpoena power to request confidential information from any given firm or plant to assess whether its separated workers qualify for TAA.[25] To qualify, investigators look for one of three criteria:

(1) Direct reduction in sales from import competition

(2) Shifts in production outside the US

(3) Upstream supplier or downstream client of firms affected by (1) or (2)

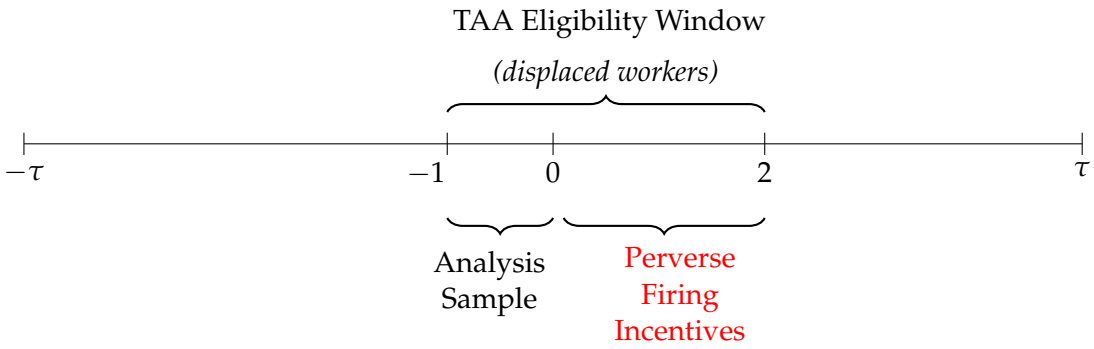
Importantly, once investigators certify a petition associated with a given plant, *all workers* displaced from that plant within a specified 3-year eligibility window automatically qualify for TAA, *despite who files*. The timeline below illustrates this point, and shows which displaced workers are eligible for TAA based on a statutory window around the petition determination date (event time $\tau = 0$). When a petition is filed, investigators record the "impact date" as the date a worker-cohort was laid off due to a trade event, which may not exceed one year prior to when the petition is filed. The "expiry date" marks the last day a worker can be laid off and still receive benefits—statutorily two years after the petition decision.

[22] Percentages from author calculations based on DOL petition data, 1974 - 2016.

[23] Figure 2 also demonstrates a countercyclical filing pattern. However this may be unsurprising, as firms have been shown to make structural adjustments precisely during recessions (Aghion et al., 2005).

[24] The short TAA petition form can be found here: https://www.doleta.gov/tradeact/docs/RevisedPetition.pdf

[25] Investigators do this by issuing Confidential Data Requests: https://www.doleta.gov/tradeact/pdf/CDR_Service.pdf, and typically arrive at a determination decision within 60 days.



$\tau = -1$: TAA "Impact Date" ($\approx$ Displacement Date)
$\tau = 0$: TAA Determination Date (Approve/Deny)
$\tau = 2$: TAA Eligibility Expiry Date

In corresponding Census microdata, I thus identify my sample as all "displaced" workers that move from positive to non-positive earnings at a petition-associated plant, within the TAA eligibility window.[26] One unintended consequence of this eligibility design is that the plant itself may become aware of its workers' TAA approval status after the decision is made public at $\tau = 0$. If so, firms with union-represented workforces or those located in "company-towns" may have an incentive to fire *additional* workers who were originally retained after the first wave of layoffs, with the knowledge that these workers will be compensated.[27] Because retained workers are presumably selected for higher productivity and would only be laid off if approved for TAA, this could contaminate the treatment group and bias earnings estimates upwards. I thus only include workers laid off in the one year prior to the petition decision. Despite this relatively conservative condition, I am still able to identify a final sample of roughly 300,000 displaced workers after sampling restrictions.

While the treatment is effectively at the plant-level, this does not guarantee that information about TAA eligibility disseminates to all qualified workers. However, it does allow for the estimation of an intent-to-treat (ITT) estimator which reflects the effects of qualifying for TAA despite potential program shortcomings such as information asymmetries. Indeed, Schochet et al. (2012) estimate that 25-45% of workers offered TAA benefits actually take up the program. In combination with the additional restriction above to preclude perverse incentive problems, these are important limitations when interpreting local average treatment effects and weighing program benefits against costs.

[26] The main results are robust to a number of sensitivity samples that use alternative definitions of displacement that more closely follow the "mass layoff" literature. These are discussed further in the sample restrictions outlined in Section 3.

[27] Whether the motive is profit or non-profit rooted, as noted above, most petitions are filed by companies instituting layoffs rather than workers or unions.

2.3 TAA Training Programs

The Department of Labor classifies training by three main categories: occupational (vocational, trade schools, community college, associates degree, etc.), remedial (elementary proficiency), and on-the-job (training at the firm from which the worker separated with the expectation of rehiring after training).[28] According to TAA performance data from 2000 to 2016, 62% of TAA workers enrolled in occupational training programs, 18% in community college and credentialed programs, 15% in remedial programs, and less than 1% in on-the-job or apprentice training programs (Trade Act Participant Report).

For a subset of this data (2001 to 2007), I am able to observe the most targeted post-training occupations and most frequent occupational transitions using DOL-ascribed occupational codes (O*Net 4.0 8-digit codes). Consistent with expanding industries during this time period, the most targeted occupations from TAA training were computer operators, office clerks, transcribing-machine operators, medical assistants, and nursing aids. In terms of occupation switching, some workers choose training programs to deepen industry-specific human capital, while others appear to enroll in training to expand more general human capital. For example, two of the most commonly displaced occupations during this time period were *commercial aircraft workers* and *men's clothing workers*. Many commercial aircraft workers retrained in areas that seem like natural transitions—becoming tractor operators and air conditioning or refrigeration mechanics. However, another popularly targeted occupation among this same group was medical assistants. Men's clothing workers on the other hand, frequently retrained to become sewing machine operators—choosing almost exclusively to deepen knowledge in their own industries. While these reflect some prominent examples of targeted switches, this does not inform us as to whether such transitions were actually successful.

3 Data Sources and Matching

This project uses two main administrative datasets, merging the history of TAA petitions to U.S. Census Bureau administrative data.

3.1 TAA Historical Petition Database

I have first acquired the universe of approved and denied TAA petitions (1974-2016) through Freedom of Information Act (FOIA) requests at the DOL. This dataset contains an observation for each petition (roughly 84,000 in total),[29] and provides three critical pieces of information.

[28] While firms can be reimbursed directly for providing on-the-job training to laid-off workers, this comprises a very small share of overall TAA training activities.

[29] This includes about 8,400 petitions for the NAFTA-TAA program described in Section 2.

1. First, each petition contains the plant (establishment) name and address, which I leverage for matching to Census Bureau establishments and the workers employed at those establishments.

2. Each petition contains a series of dates, including the petition filing date, determination (TAA approval decision) date, impact date, and eligibility expiry date (see Section 2 for definitions). These are used for identifying and pulling the set of workers laid off in the eligibility window.

3. Crucially for the paper's main identification strategy, each petition also contains the last name of the investigator assigned to each case. These names were intensively cleaned in close consult with DOL officials and validated with historical employee records.[30]

The petition database additionally includes information on petitioner filer types (company, union, worker-group, or state career center), DOL-assigned 4-digit Standard Industrial Classification (SIC) codes, the company's main product or service (recorded as a qualitative value), and an indicator for whether the petition was designated as part of the NAFTA-TAA program, TAA, or both. Finally, each petition contains an estimate of the number of workers covered—a 3-year estimate spanning the entire eligibility window discussed above, that assumes that every laid off worker would take up the program.

Filers come from a variety of industries, from well-known automobile manufacturing plants in Michigan, to displaced port workers in Seattle. Table 1 shows the location of the top TAA filing zip codes and the industries associated with those zip codes in each decade of the program's existence. Moving from 1975 to 2016, filing has gradually moved from manufacturing industries in "Rust Belt" states to a more disperse set, both geographically and in terms value-added activities.

3.2 Construction of LEHD Worker-Level Panel

I merge TAA petition data to the US Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) administrative files. The LEHD files allow for the construction of a detailed person-level panel dataset which tracks quarterly worker earnings, labor force participation (employment), and educational status across employers, geographies, and time. The core data are compiled from employer-reported Unemployment Insurance (UI) filings at the state-level for every paid employee. While the LEHD data partnership spans all 50 US states and covers over 90% of US workers,[31] states approve researcher requested data on a state-by-state basis. For this project, 24 states and the District of Columbia approved data access.[32] I use the 2011 LEHD snapshot which provides a relatively balanced panel from 1990 to

[30] This was needed to calculate precise investigator leniency values. For example, this included tasks such as identifying whether "Green" and "Greene" were the same or separate investigators.

[31] As part of the non-included 10%, the LEHD excludes workers who are self-employed, agricultural workers, and some government employees. See Abowd et al. (2009) and Vilhuber and McKinney (2014) for further details.

[32] These include AR, AZ, CA, CO, DC, DE, FL, IA, ID, IL, IN, KS, MD, ME, MO, MT, NM, NV, OK, OR, PA, SC, TN, WA, WV.

2011 (see Appendix A.1 for further details). Leveraging each worker's (de-identified) social security number, I merge in worker age, gender, and race, from the Social Security Administration Numident file (available in the LEHD Individual Characteristics File). Educational attainment variables are also available for Decennial Census and American Community Survey (ACS) respondents (roughly 1 in 6 workers), while education imputes based on Census Bureau multiple-imputation and probabilistic record linking methods are used for non-respondent workers.[33] Education can take four ordinal values: 1=Some High School; 2=High School Degree; 3=Some College; 4=College Degree.

There are some important caveats imposed by the data limitations discussed above. Figure 3 compares the number of estimated TAA workers covered by filed petitions in all 50 states (cumulated over the entire TAA history from 1974 to 2016), against the number of filers in the 24-state LEHD sub-sample from 1990 to 2011. These are mapped to 1990 Commuting Zone (CZs) geographies, which are the central unit of analysis when measuring the effects of TAA on spatial mobility.[34] The figure reveals that while filers are generally concentrated in "Rust Belt" and southern manufacturing states, the LEHD sub-sample is missing some notable political swing states such as Michigan, Wisconsin, and Ohio (however contain others such as Pennsylvania).

Secondly, as is the case in most labor settings however, the LEHD cannot distinguish between a number of cases that may be the cause of a missing earnings value. Following standard practice, I designate a worker as unemployed if she has a missing earnings value between two positive earnings observations. However I cannot discern between unemployed workers versus those who are retired, deceased, reliant on extended take-up of social insurance programs such as disability insurance, self-employed, or discouraged from the labor force. Importantly, I also cannot know whether a worker with a missing earnings value is in fact employed in a state *outside* of the set of 24 approved for this project. This limitation would be most problematic for mobility estimates. In robustness tests, I thus show that the main effects are stable when using the more contiguous West Coast states shown in Figure 3 which are less likely to feature mobility outside of the region.

3.3 Matching TAA Petitions to Displaced Workers at LEHD Employers

To be able to detect a potentially small effect size of TAA in noisy earnings data, I desire as conservative a matching strategy as possible. To identify TAA filers at the worker-level, I first have to identify both the correct TAA petitioner plants and the timing of the separation event in the LEHD.

[33] All effects of TAA on formal education are robust to using both imputed and non-imputed measures. It also bears noting that while worker occupations are available for ACS respondents, these data were not made available for the current draft however will be explored in future work.

[34] Using the definition from Dorn (2009), "Commuting Zones (CZs) provide a local labor market geography that covers the entire land area of the United States. CZs are clusters of U.S. counties that are characterized by strong within-cluster and weak between-cluster commuting ties."

Beginning with identifying TAA plants, the 2011 LEHD snapshot contains each establishment's State Employer Identification Number (SEIN) and federal Employer Identification Number (EIN), however does not directly report business name and address.[35] Using commingled IRS and Census data from the Business Register—the Census Bureau's most comprehensive database of U.S. business establishments (formerly Standard Statistical Establishment List)—I am able to assign a company name and address to each EIN in the LEHD, and subsequently match LEHD plants to TAA petitioners on cleaned company names and addresses.

I keep only the first TAA petition associated with any given plant so that no worker is assigned to both the treatment and control group due to multiple filings. I then match TAA plant addresses to Business Register addresses in both the calendar year of the petition and the year prior to the petition (to account for any potential filing lags or measurement error in filing dates).[36] Finally, I keep matched LEHD establishments if the plant address is unique to a given state-year cell. By focusing on one-to-one matches, this circumvents problems arising from matching one TAA establishment address to a Census location containing many establishments at the same address (such as an office building). The sample should thus be interpreted as representative of rural, stand-alone, manufacturing plants. I then repeat this exact same procedure for company names, extensively cleaned in both datasets.

Drawing three random samples of 100 TAA petitions, I manually cross-check company names and addresses and verify a match rate of 98%, with the residual likely owing to discrepancies between parent and subsidiary names which could not be reconciled between the two datasets. This results in a final matched sample of 4,700 unique TAA establishments—comprising roughly 13% of plants associated with TAA claimants from 1990 to 2011. Finally, each matched plant's SEIN number can then be used to pull all workers employed at those plants during the eligibility window described in Section 2.2. The final step is to pull the full job history for each of these workers, and identify which workers separated or were "displaced"—defined here as moving from positive non-positive earnings during the TAA eligibility window.[37] For further details on this procedure, please see Appendix A.1.

3.4 Sample Restrictions and Summary Statistics

Having identified TAA-eligible displaced workers associated with both approved and denied petitions, I implement a number of sample restrictions to focus on those who are most likely to take up training were they randomly assigned TAA approval. As noted in Section 2.2, I keep workers only if they

[35] Each EIN and SEIN may have multiple and distinct units. For details on how this is dealt with, see Appendix A.1.

[36] I build a comprehensive address cleaning file based on other papers, including code generously provided by Monarch et al. (2017) who also match TAA plants to Business Register addresses.

[37] Workers can furnish returns from multiple employers, but earnings are aggregated (collapsed) such that each worker only has one earnings observation per quarter.

were displaced in the year prior to the petition decision, which eliminates one third of the original sample. I further restrict attention to working-age individuals (22-65) who make less than \$50,000/year ($\sim$85th percentile) in all years prior to the petition filing. This value is a commonly used threshold for Department of Labor programs, and eliminates the spurious inclusion of CEOs, managers, and other workers who are both unlikely to take up training and not the population of interest in this study.

Quarterly earnings are deflated to 2010 real dollars using a seasonally adjusted consumer price index. For computational tractability, I further collapse worker earnings to the year relative to the petition decision (event time τ). While workers can furnish returns from multiple employers, earnings are aggregated such that each worker only has one earnings observation per relative year (the unit of analysis). I include observations 10 years before and after the petition decision such that $\tau \in [-10, 10]$.[38] These restrictions reduce the overall dataset from $\sim$25 million to $\sim$4.2 million observations and 287,000 workers (rounded as per Census Bureau disclosure requirements). Earnings and employment values are left- and right-censored within each worker panel, however the main paper results are robust to both censoring and replacing edge values with zeros.

Lastly, for comparability with the labor literature, I define a "High Labor Force Attachment" sub-group as the preferred analysis sample of the paper. This includes workers with at least 8 quarters of positive earnings in the pre-period at or above the full-time minimum wage equivalent (\$7.25 * 2,082 average working hours in 2010 $\approx$ \$15,000 annually).[39] This results in a final sample of 177,000 displaced workers. However, all main results are robust to using both the high attachment sub-sample and the unrestricted sample.

Table 2 shows summary statistics for both groups, pooled across all periods (pre-period balance statistics are presented in the next table). Starting with panel A, workers in both samples are largely middle-aged, predominantly white, and slightly skewed toward being female. Worker high school and college graduation rates are almost exactly in line with national averages for the time period.[40] Because they are included based on a separation event, worker earnings and quarters employed per year are expectedly below national averages, with the average worker making \$22,830 and \$18,814 in each sample respectively (see panel B). Workers have on average about 1 job, however there is large dispersion in the worker's total number of employers in a given year. As indicated by the county unemployment rate at the time of filing, these workers are in highly distressed local labor markets with unemployment rates near 6.5% (BLS Local Area Unemployment Statistics).

[38] This window takes maximal advantage of the 21 years of data spanned by the LEHD.

[39] Further restrictions on the petitioner side include keeping only those petitions assigned to investigators with more than 10 cases, to eliminate potential noise associated with investigator leniency rates calculated from small cells.

[40] Ryan and Siebens (2012) show that post-1990 high school graduation rates were roughly 80%—consistent with adding the three education values in the summary statistics—whereas college graduation rates were roughly 25%.

I also report overall mobility rates expressed as the likelihood a worker remains at the firm, industry, or commuting zone of the employer from which they initially separated. Regarding petitioner characteristics (panel C), companies and worker-groups are the most common filers, followed by unions. It bears noting that approval rates are much higher when companies file with respect to non-company filers, which can be seen by comparing "TAA Approved" versus "TAA Denied" groups in either sample.

Both samples contain roughly 250 case investigators during this time period, who remain on average for about 10 years with the Department of Labor and handle roughly 23 TAA cases over that tenure. To see the extent to which approval rates map to standard measures of industry tradability and occupational offshorability associated with those industries (presumably known to investigators), I merge in three known indices from Mian and Sufi (2014), Autor and Dorn (2013), and Blinder (2007), which show that higher tradability values are generally associated with the approved TAA groups. For details on their construction at the industry-level, see Appendix A.2.

4 Empirical Framework

4.1 Limitations to Difference-in-Differences

The top panel of Figure 4 plots raw means for TAA approved and denied workers across their separation events, relative to the year of the petition decision ($\tau = 0$). At first glance, one might look at this figure and conclude that workers who are approved for TAA are no different than those that are denied in the pre-period. The difference-in-differences (DID) common trends assumption appears to hold visually, and thus we are tempted to interpret the post-period (lack of) differential earnings response as a causal effect. However this is highly misleading for at least two reasons.

First, these means mask incredible heterogeneity in both levels and pre-trends. In some industries like textiles, TAA approved workers have both differentially declining earnings and a lower level of overall earnings relative to non-TAA approved workers, which might bias TAA effects downwards. Whereas in other support industries such as aluminum manufacturing products and auto parts, TAA-approved pre-treatment earnings are much higher than non-TAA earnings, which might bias TAA effects upwards.[41] The point here is that there is no good reason that approved workers should look like denied workers *absent* TAA. Secular trends for workers at "tradable" industries confound post estimates.

Second, even if the common pretrends assumption held, TAA workers may respond *differentially* to a lay-off event relative to non-TAA workers. Those working at non-tradable industries may for example, reflect certain localized high value added jobs with skills more easily transferable to other

[41] In additional results, I show distributions of differential levels and pre-trends across industries to highlight violations of the DID identifying assumption.

sectors. In this case, one would need an even stronger identifying assumption such as parallel *post-trends* absent the treatment. Workers may be different for a number of reasons correlated with tradability, geography, demographics, or heterogeneity in access to types of training, yielding DID estimates that are not credible.

4.2 Investigator Assignment and Leniency IV

> *TAA cases are assigned [to investigators]* ***primarily*** *based on investigator caseload, as well as previous experience with a company or industry. Staff leave or other scheduling issues can be a factor as well.*
>
> —Correspondence with Office of Trade Adjustment Assistance,
> (obtained via Department of Labor FOIA request, 12/15/2016)

To overcome the challenging selection problems involved in identifying the causal effects of TAA, I take advantage of how petitions are assigned to case investigators. I define an investigator's leniency as the share of total cases approved over their entire tenure, less the current case (excluding those with less than 10 cases over their tenure).[42] This "leave-one-out" leniency measure thus varies by investigator and case, but is time-invariant.[43] According to the assignment mechanism described in the quote above, TAA assigns two otherwise identical worker cohorts displaced from the same industry, different TAA approval probabilities based on whether their case is directed to a more *lenient* versus *strict* investigator.

I thus use this leave-one-out measure of investigator leniency as an instrumental variable (IV) for assignment to TAA.[44] Yet as leniency may still be correlated with petitioner characteristics through industry specialization and experience, I must also include a number of flexible controls for each investigator's industrial concentration and experience. Conditional on the successful inclusion of these controls, assignment based on caseload should effectively be as if randomly assigned.

The intuition for how this instrument alters earnings results with respect to the naive DID estimator, is shown in the bottom panel of Figure 4. Raw earnings are plotted against top and bottom leniency quartiles, making this an unconditional version of the "reduced form equation" of a corresponding 2SLS procedure (see figure for quartile cut-offs). Already using just raw means for workers assigned to lenient (top quartile) versus strict (bottom quartile) investigators, we begin to see earnings patterns separate

[42] In some cases, the petition also lists the last name of the certifying officer who assigned the case. For those cases, I calculate leniency across investigator-certifier cells, however all results are robust to using just investigator leniency as well as investigator-certifying officer measures.

[43] Additional results show that investigator leniency is relatively stable within investigators over time. DOL investigators do work on a number of cases beyond those associated with TAA, including for example Occupational Safety and Health Administration (OSHA) cases. However these are not observed in the data and thus not included in the leniency measure.

[44] While investigator leniency is the most, Belloni et al. (2012) propose a lasso method for selecting the most strongly binding among a number of plausibly exogenous instruments. However in this paper, the only other investigator-level variable available is an imputed investigator race variable based on last name, and I thus do not employ this method.

in the post-period. This is, in fact, the variation underlying the main results of the paper. While the figure still exhibits some pre-trend differences, these are eliminated when including the flexible controls mentioned above.

4.3 Estimation Strategy

I employ a two-stage least squares (2SLS) estimation strategy that tracks workers as they move across employers using repeated (pooled) cross-sectional regressions over event time:

$$TAA_{ijkt} = \theta Leniency_{j(i)}^{-p} + \alpha C_{jkt} + \delta N_{jkt} + \lambda_k + X_{ijkt}^{'}\gamma + \nu_{ijkt} \tag{1}$$

$$Y_{ijkt\tau} = \beta_\tau TAA_{ijkt} + \underbrace{\alpha C_{jkt} + \delta N_{jkt} + \lambda_k}_{\text{restoring randomness}} + X_{ijkt}^{'}\gamma + \epsilon_{ijkt\tau} \tag{2}$$

In the first stage, worker i's petition is assigned to investigator j in calendar year t. TAA_{ijkt} is an indicator variable equal to 1 if that worker is associated with an approved TAA petition. $Leniency_{j(i)}^{-p}$ measures the total share of cases approved by investigator j less the worker's own petition p.[45] k indexes the 4-digit SIC industry from which the worker was displaced, and $\tau \in [-10, 10]$ is the year relative to the petition approval decision at $\tau = 0$. Outcomes $Y_{ijkt\tau}$ include worker earnings, quarters employed, formal education, and the probability workers move 2-digit NAICS industries and commuting zones.

I include three assignment variables to control flexibly for differential approval propensities among highly specialized or experienced investigators. N_{jkt} is the raw number of cases that an investigator j has adjudicated in industry k by calendar year t. $C_{jkt} = \frac{N_{jkt}}{\sum_j N_{jkt}}$ is the *concentration* of industry k cases assigned to investigator j by time t. While these may be correlated, they are not perfectly collinear due to the fact that investigators are not balanced across the entire panel. In both stages, X_{ijkt} are precision controls that are mostly time-invariant and measured in the baseline (pre-application) period. The exception to this is a polynomial in age and variables interacted with that polynomial, however these vary mechanically over time and cannot be endogenously manipulated in response to the TAA treatment.[46]

The coefficients of interest are each β_τ, which are estimated in separate regressions for each τ. When overlaid on an event-study style plot, these point estimates dynamically map out the effects of TAA over time. Thee include placebo estimates for all $\tau < 0$, which test for the effects of TAA prior to the worker's separation and petition for benefits (i.e. periods in which we would not expect to find any statistical significant differential effects of TAA).

[45] I use the subscript *j(i)* to clarify that worker *i* is assigned to investigator *j* (*j* for "judge").

[46] Not shown in equations (1) and (2) but indicated clearly in regression tables, calendar-year and filer-quarter fixed effects are included in most specifications.

4.4 Randomization Tests and IV Assumptions

Like all instrumental variables, the leniency IV must satisfy two main conditions: excludability and relevance (including monotonicity).[47]

Excludability

While excludability cannot be tested directly, I present two strong pieces of evidence supporting the claim that investigator leniency is orthogonal to pre-determined characteristics of petitioners (conditional on concentration controls), and that the instrument only affects worker outcomes through assignment to TAA.

In Table 3, I test for random assignment of TAA petitions to DOL investigators by regressing the instrument on pre-determined characteristics. If investigators were assigned to cases with particular economic or demographic characteristics, we would expect to observe statistically significant predictors of higher leniency values. However, as we move through the columns from left to right, consistent with randomization, very few covariates are predictive of the instrument. (When they are statistically significant, the coefficients are economically so close to zero that they can be ignored.)

Starting with column (1), I report the baseline mean of all pre-determined characteristics prior to the eligibility window (5 to 40 quarters before the petition decision inclusively). The dependent variable in column (2) is the endogenous TAA approval indicator variable. Consequently, it is not surprising that many of the coefficients in this column are statistically significant. Notably, in panel C, company filers have approval rates that are on average 22 percentage points higher than the omitted filer group (state career centers), and TAA approved workers are more likely to be considered "tradable" according to the Mian and Sufi (2014) definition.

The dependent variable in columns (3) to (5) is the investigator leniency IV. Columns (4) and (5) add the three "randomness restoring" controls shown in the main specification, where *investigator concentration controls* include both C_{jkt} and N_{jkt} measures (defined above). Adding these controls only alter randomization results in minor ways—consistent with most variation being driven by caseload rather than industry-based assignment. Since the regressions presented here each include many covariates however, one may be concerned that the oversaturation of the model with controls is what drives the zeros, rather than random assignment. Column (6) thus shows p-values from separate regressions of the leniency IV on each covariate independently. Here, as in all regressions, standard errors are clustered at the investigator level (the level of randomization). Finally, all regressions contain

[47] Technically there are three separate assumptions including monotonicity, which is difficult to test explicitly. In additional results, heterogeneous subgroups of the analysis all contain a positive first stage such that higher leniency scores weakly increase TAA probability, facilitating a local average treatment effect interpretation for compliers.

quarter-of-filing and calendar-year fixed effects.

The second piece of evidence that assignment based on caseload is as if random, is that investigators with low caseloads do not systematically differ in their leniencies from investigators with higher caseloads. Figure 5 demonstrates non-parametrically, how neither investigator caseload nor tenure at the Department of Labor are correlated with leniency. This rules out confounding stories such as cases being assigned to more experienced investigators who regularly approve certain types of workers, or the idea that lower caseload workers have different competencies which could be correlated with pre-determined characteristics of the types of workers they approve—violating the exclusion restriction.

Relevance and First Stage

Figure 6 displays variation in the investigator leniency measure pooled across the program's history. The figure plots the number of petitions filed across 50 bins of leniency, and exhibits a surprising degree of variation. The plot is then overlaid with two kernel densities to express the distribution of leniency's stability over the business cycle.

Table 4 reports companion first stage estimates corresponding to equation (1) of the paper for the "High Labor Force Attachment" sample. Column (1) includes the minimal set of controls needed for identification, while moving left to right adds precision controls and investigator characteristics (which have already been shown to be uncorrelated with approval rates in Figure 5.) *Baseline Controls* include means over 5 to 40 quarters prior to the TAA petition decision for the following variables: worker earnings, quarters employed, worker tenure, and initial employer and state earnings. *Full Regression Controls* reflect covariates estimated in the preferred specification in subsequent tables, and include: race and gender fixed effects, a cubic in age fully interacted with baseline earnings and gender indicators, filer-state fixed effects, filer-type fixed effects (company, union, worker-group), and a dummy variable for whether a petition applied as part of the NAFTA-TAA program.

Interpreting the magnitude of the first stage, a coefficient of 0.60 can be interpreted as a 10 percentage point increase in leniency (the share of cases approved) being associated with a 6 percentage point higher approval rate. I report the F-statistic from an F-test on the excluded instrument, which is consistently strong and robust despite conservative clustering. Finally, the data shown here use the main analysis sample at the worker-level. When regressions corresponding to models (1) and (2) are estimated at the petition-level using just petition data however, the first stage coefficient is closer to 1 (which is consistent with the examiner literature).[48] Overall, these results suggest that the instrument indeed has a large amount of predictive power over worker approval for TAA.

[48]There is also a large increase in the excluded IV first stage F-statistics at the petitioner-level.

LATE Compliers

Before proceeding to the main results of the paper, it is worth highlighting which types of petitioners and workers are expected to comply with the treatment. Each TAA approval decision can be thought of as a latent variable sum of an observable "tradability" signal σ and subjective adjudication noise ξ^j for each investigator j (see Dobbie and Song (2015) for a similar set-up). This is expressed in Appendix B.1. The left panel orders mean industry approval rates from lowest to highest by 3-digit NAICS codes, calculated over all TAA cases from 1974 to 2016. These are highly correlated with known measures of tradability. The right panel shows within-industry investigator variation around the same mean approval rates, where the median is indicated with a black dot and range plot whiskers reflect the 35th to 65th leniency percentiles within each industry.

The high-variance industries along the middle bracket of the support reflect the "marginal" cases that are hard to adjudicate, and for whom the randomly assigned investigator is most likely to be influential. Roughly 80% of workers are assigned to investigators with leniency rates between 0.2 and 0.8, which suggests that indeed most TAA applicants are not part of industries that are easily assessable as having been impacted by offshoring or import competition. This raises an important concern about the extent to which the results remain relevant to trade-shocked workers. If indeed industry tradability is hard to adjudicate, the local average treatment effect may in fact represent the effects of training on a different population rather than workers most adversely affected by trade. For example, if trade shocks are correlated with technological upgrading or automation, the LATE will reflect some combination of these. In additional results, I parse estimates by different regions of this support and find no significant differences across low and high values of tradability.

Lastly, it is important to note that the LATE estimated here is in fact an intent-to-treat estimator which mechanically understates the treatment-on-treated or "true" LATE. Without detailed take-up data at the individual or plant-level however, one cannot conduct the desired bounding or decomposition exercises that would help place these effects in a more general context, nor use intent to instrument for take-up as is common with one-sided attrition models.

5 Main Results

5.1 Impact of TAA on Worker Earnings, Employment, and Formal Education

Table 5 reports the main effects of TAA on worker earnings using the regression specified in equation (2), where event time τ is pooled across two periods (such that each row represents a separate regression):

Pre-Training: $\tau \in [-10, -1]$; and Post-Training: $\tau \in [2, 10]$.[49] Starting with the post period, column (1) shows baseline OLS estimates that are negative and statistically significant, suggesting that TAA workers receive on average $964 lower annual earnings after training. However, this estimate is not credible for the myriad of reasons discussed above. When applying the leniency instrument in a 2SLS approach in columns (2) through (6), the effect sign and magnitude dramatically change. Column (6) shows the preferred TAA earnings estimate of the paper. Accordingly, TAA takers have on average $10,256 higher annual earnings, relative to all-else-equal non-takers.[50]

Successively adding precision controls from columns (2) to (6) only minorly alter effect sizes. Particularly influential in reducing standard errors are controls added from columns (4) to (6). These are potential signs that TAA effects may have important dimensions of heterogeneity by filer type, local labor market (including states), and demographics. Lastly, consistent with prior randomization results, the pre-training period placebo estimates consistently show a precise zero. That is, there are assuringly no differential earnings effects of TAA when we not expect there to be (prior to the worker's separation).

Considering average baseline earnings prior to layoffs were $26,880 (Table 3), an effect size of $10,256 implies a relatively large elasticity. To further understand these results, I reestimate equation (2) separately for each period 10 years before and after the year of the TAA approval decision. The results for these 21 regressions (using the preferred specification from column (6) of Table 5) are plotted in the top panel of Figure 7, where vertical lines partition the support into pre-TAA, during-TAA, and post-TAA periods. The figure shows a clear and surprisingly pattern. Expectedly, workers inferred to take up benefits forego roughly $10,000 in income while training for two years. Perhaps unexpectedly however, large initial returns decay over time. Annual incomes among TAA and non-TAA workers in fact, fully converge after ten years. Further tests reveal that this pattern is not driven by differential attrition.

Importantly, the dependent variable here includes earnings values equal to zero when a worker is unemployed, and is thus reflective of the joint earnings *and* labor force participation effect. To begin to decompose these two mechanisms and rationalize the pattern of depreciation, the bottom panel of Figure 7 implements the same estimation but calculating the dependent variable as within-worker cumulative earnings from $\tau = -10$ to the event year on the x-axis. The results from these cumulative

[49] As the vast majority of program participants enroll in two-year programs or less, I conservatively use this window (rather than a 3-year training window) as the treatment period. For effects on earnings *during* training, see Figure 8 which shows more granular annual effects over event time.

[50] Variable definitions are analogous to those shown in Section 4.4. *Baseline Controls* include means over 5 to 40 quarters prior to the TAA petition decision for the following variables: worker earnings, quarters employed, worker tenure, and initial employer earnings. *Full Regression Controls* reflect covariates estimated in the preferred specification in subsequent tables, and include: race and gender fixed effects, a cubic in age fully interacted with baseline earnings and gender indicators, filer-state fixed effects, filer-type fixed effects (company, union, worker-group), and a dummy variable for whether a petition applied as part of the NAFTA-TAA program. All specifications include the identification controls needed for restoring randomness according to equation (2), and standard errors are clustered at the investigator level (the level of randomization).

earnings regressions suggest that indeed, effects are being driven by both earnings as well as labor force participation. The advantage to expressing earnings this way instead of logging, is that one does not need to be concerned with logging earnings values of zero. Appendix B.3 shows however, that logging produces very similar results. Furthermore, cumulative earnings have an appealing interpretation. Ten years out, workers have approximately $50,000 higher cumulative earnings relative to all-else equal workers that do not retrain. This pattern is fully stable to using the larger "full" sample which does not only restrict attention to workers that were more highly attached to the labor force (see Appendix B.2 for the corresponding figure, and Appendix C.1 for more detailed event-year coefficient estimates.)

Table 6 reports the main effects of TAA on number of quarters employed using the same specification and controls as before. Focusing on the preferred estimate in column (6), TAA-trained workers are employed on average 0.64 more quarters each year in the post period. The top panel of Figure 8 shows the corresponding dynamic pattern of employment, which closely mirrors earnings effects. Trained workers appear immediately more employable than non-trained workers, which further suggests that there are no economically meaningful extra search costs after TAA workers exhaust extended UI benefits (a mechanism that we can thus tentatively rule out but return to when discussing mobility effects). Back-of-the-envelope calculations suggest that roughly 60% of the prior earnings pattern can be explained by this higher employment rate.[51] While the LEHD does not contain variables for hours worked, I am able to test whether TAA affects the number of jobs a worker has in a given year. If displaced workers that do not receive TAA benefits face reduced hours in the labor market, this may materialize as workers taking multiple part-time jobs. However, the bottom panel of Figure 8 confirms that there is no discernible effect on the number of jobs held.

If indeed training contributes a sizable benefit that is capitalized into earnings, we may expect to see differential take-up along formal education measures. Table 7 reports the main effects on two such measures—the probability a worker has educational attainment greater than or equal to a high school degree and "some college" respectively. As discussed before, a concerning caveat about this data is that the measurement is both ordinal and sparsely populated. I show however in the table, that across all specifications, there is no statistically significant effect on the probability that a worker moves up the formal education scale. This is robust to using the constrained sample for which education values are not imputed, and the full sample included education imputes. Figure 9 demonstrates this clearly.

[51] Using denied worker annual earnings as a baseline ($23,468 from Table 2), I divide this by 4 to get quarterly earnings, and multiply by 0.64 to attain the relative earnings return for each year in the post period. I then calculate this as a fraction of the cumulative $50,000 earnings return just established.

5.2 Heterogeneity and Mechanisms

The fact that TAA appears to increase earnings, that those earnings depreciate, and that there are no effects on formal education (such as community college take-up), together suggest that TAA may only augment short-run demanded human capital rather than the types of permanent capital that are known to have more durable returns (Card (2001); Kane and Rouse (1995)). While there is suggestive evidence that most effects are coming through training and not extended unemployment components of TAA, there is also concern that trained workers are just getting bumped to the top of local labor market job queues at the expense of other workers.[52] One way this might manifest itself is if trained workers stay in the same industries and labor markets relative to untrained workers. Conversely, if TAA workers switch industries and labor markets, this is more consistent with an adjustment friction story rather than labor market crowd-out.

To begin to test these mechanisms, I expand the main regression specification of the paper to account for simple heterogeneity in pre-TAA characteristics such that regression coefficients reflect within-group causal effects. In the specification below, I interact the main effects of TAA (mediated by investigator leniency) with "High" and "Low" values of pre-treatment covariates in both stages.

$$TAA_{ijkt} * 1(High)_t = \theta_1^H Leniency_{j(i)}^{-p} * 1(High)_t + \theta_2^H Leniency_{j(i)}^{-p} * 1(Low)_t + ... + \nu_{ijkt}^H \quad (3)$$

$$TAA_{ijkt} * 1(Low)_t = \theta_1^L Leniency_{j(i)}^{-p} * 1(High)_t + \theta_2^L Leniency_{j(i)}^{-p} * 1(Low)_t + ... + \nu_{ijkt}^L \quad (4)$$

$$Y_{ijkt\tau} = \beta_{1\tau} TAA_{ijkt} * 1(High)_t + \beta_{2\tau} TAA_{ijkt} * 1(Low)_t + ... + \epsilon_{ijkt\tau} \quad (5)$$

The above specification is analogous to equations (1) and (2), where equations (3) and (4) correspond to the new first stage. Ellipses (...) are shown to parsimoniously capture exogenous controls $\alpha C_{jkt} + \delta N_{jkt} + \lambda_k + X'_{ijkt}\gamma$ in all three equations. Unlike before however, I include $1(High)_t$ and $1(Low)_t$ as indicator variables for whether a worker is above or below the median of a certain pre-treatment covariate. In this way, one can plot the main coefficients of interest across two simple heterogeneity groups ($\beta_{1\tau}$ and $\beta_{2\tau}$) to test for heterogeneous treatment effects.

Training Quality and Duration

I begin by examining heterogeneity in training quality. While quality is unobservable, the Department of Labor Trade Act Participant Report (TAPR) records average weeks trained for TAA participants on a quarter-by-state basis, which varies based on fiscal resources committed to vocational training and

[52] See Heckman, Stixrud, and Urzua (2006) for a nuanced view of how schooling and job signaling may play out in such scenarios.

state education programs. Using duration data from 2001 to 2016, I define "High" and "Low" training states with respect to the median weeks trained per person in each calendar quarter. Figure 10 shows the overlaid results of the two coefficients of interest, suppressing standard errors for clarity (however statistical significance is reported in Table 8).

Remarkably, workers that are located in high-duration states prior to their layoff (the series marked with an "x") do not exhibit the depreciating pattern that is common to those in lower training duration states. While one might be concerned that in fact, lower training duration reflects drop-out rates or other selected worker qualities, it is unlikely that an entire state features large enough selection effects to explain this difference. In conjunction with previous evidence, I thus attribute the depreciation to short-run demanded skills becoming obsolete (consistent with rapid skill-biased technological change or an overall declining labor share).[53] Indeed, 62% of TAA training programs confer vocational degrees with shorter program lengths than typical community college or 4-year college degrees. Whether these vocational programs are responsive enough to more rapidly adapt to changing demand, or if instead more human capital investment is needed to catch up to the frontier, requires more research. However the results here provide some strong leads for future work in this area.

Effects on Spatial and Industrial Mobility

To analyze effects on worker mobility across commuting zones and industries, I first rank counties by the severity of their shock based on Bureau of Labor Statistics (BLS) unemployment rate data (Local Area Unemployment Statistics), which I merge to the LEHD panel. I define high and low shock severity regions based on whether the county in which a TAA-qualified worker was displaced was above or below median unemployment in their quarter of separation.[54]

Figure 11 reveals that workers displaced in slacker labor markets (higher unemployment counties) have returns that are almost double those of low unemployment rate counties. Foregone earnings are higher for workers in high-shock regions, which suggests they train for longer. However, the vast majority of workers are located in high-shock counties, which is also evidenced by the summary statistics. I thus interpret the main effects as being driven by high-shock regions, and subsequently examine high and low shocks separately when analyzing mobility effects.

Figure 12 shows the main mobility results of the paper, where corresponding standard errors are suppressed but shown in Table 8. I find that workers in highly disrupted regions are more likely to switch both industries *and* commuting zones in response to training. Workers are approximately 30 percentage points more likely to move commuting zones and 50 percentage points more likely to

[53] For a discussion of the potential determinants of the declining labor share in the United States, see Autor et al. (2017).
[54] All results here are robust to using a level measure of local labor market distress, or a change over time.

switch industries (at the 2-digit North American Industrial Classification System (NAICS) level), with respect to the location and industry of their pre-layoff employer. These are large effects, when compared with baseline mobility rates of 0.26 and 0.23 for commuting zones and 2-digit industries respectively (subtracting baseline estimates in Table 3 from 1.)

While not as well identified due to potential selection concerns (but nonetheless supported by common pre-trends), I also present suggestive evidence that positive earnings returns among "movers" drive the overall effects relative to "stayers". I define movers and stayers by their *ex-post* mobility decisions, which may reflect a self-selected sample that differs along a number of unobservables. Figure 13 shows that the main earnings results of the paper map very closely to effects for movers.[55]

Overall, while there is suggestive evidence that TAA-trained workers previously were bound by adjustment frictions, the analyses above are not explicit tests for externalities. Whether TAA is effectively expanding a feasible match radius or alleviating an informational friction remains prime ground for future study.[56] If in fact these workers are facing trade shocks which are spatially correlated (and industrially concentrated if sufficiently agglomerated in those regions), this might also explain why earnings returns are especially strong among movers. These workers would need to have much larger search radii to find productive employment.[57] Whether these strong mobility responses reflect matching or information frictions, liquidity constraints (Chetty, 2008), or both, also remains to be tested.

6 Cost Effectiveness

While the paper's identification strategy provides robust evidence of positive earnings returns and higher mobility associated with TAA, this does not inform us about the social cost-effectiveness of the program. Toward this second end, I compare the ten year stream of estimated TAA earnings returns as benefits, with average TAA expenditures on training, extended UI, and foregone earnings while training, as costs. I incorporate two important potential sources of deadweight loss (DWL): the excess burden from taxation ("leakage"from redistribution), and moral hazard search costs associated with extended unemployment insurance transfers. These are incorporated as follows:

[55] By contrast, Figure B.5 shows the effects for stayers, which I interpret as those who by chance happen to get the remaining jobs open in their local economies.

[56] See Shimer (2007) for a framework analyzing the roles of search and skill mismatch frictions in unemployment.

[57] In ongoing work, I am investigating this further by taking advantage of distances moved between employers.

$$\text{Benefits} = \overbrace{\sum_{\tau=2}^{\tau=10} \frac{\hat{\beta}_\tau}{(1+r)^{\tau+1}}}^{\text{Earnings Returns from TAA}} \tag{6}$$

$$\text{Costs} = \sum_{\tau=0}^{\tau=1} \frac{\overbrace{-\hat{\beta}_\tau + (1+\epsilon)\overline{Training}}^{\text{Foregone Earnings + Training Costs (\& DWL)}} + \overbrace{(\epsilon+\Omega) * \overline{UI}}^{\text{UI Transfer Costs (Moral Hazard \& DWL)}}}{(1+r)^{\tau+1}} \tag{7}$$

Here, *Benefits* are the discounted stream of relative TAA earnings returns for post-training years ($\tau = 2...10$), with $\hat{\beta}_\tau$ parameter estimates taken from Appendix Table C.1.[58] *Costs* contain two main terms. The first term is the discounted sum of foregone earnings while training ($\hat{\beta}_\tau < 0$ for $\tau \in [0,1]$) and average expenditures on training, where $\overline{Training} = \$6,181.96$ is calculated from the data shown in Appendix Figure B.4. ϵ represents the excess burden required by taxation to finance each dollar spent on training (Okun's "leaky bucket").[59] The second term is simpler because UI transfers from one agent to another only implicate social costs through excess burden ($\epsilon \in [0,1]$) and search disincentives induced by the transfer scheme ($\Omega \in [0,1]$).[60]

To simplify the analysis, I set $\Omega = 0.345$ (=0.6/1.6 following Schmieder and Von Wachter (2016)). Setting $\overline{UI} = \$13,181.96$, again taken from Appendix Figure B.4, we then solve the internal rate of return r that equates net present benefits to costs for different values of ϵ. Following Heckman et al. (2010), I show both private and societal internal rates of return to TAA for a range of excess burden values (where private returns to workers only include foregone earnings costs associated with training.)

Internal Rate of Return to TAA

Leakage Parameter (ϵ)	*Private Return*	*Social Return*
0%	49.5%	17.0%
25%	49.5%	9.1%
50%	49.5%	4.0%
75%	49.5%	0%
100%	49.5%	-3.0%

Note: Private returns assume workers do not bear any of the incidence of any Ricardian effects induced by the excess burden of taxation, hence their stability across different deadweight loss parameter values.

I estimate an internal rate of return (IRR) on TAA of between 0.0% and 9.1% for deadweight loss

[58] Note that $\tau + 1$ is in the exponent of the discount term to be consistent with prior notation. $\tau = 0$ should be thought of as year one, such that the social planner is positioned at $\tau = -1$ when calculating the IRR.

[59] This first term reflects the opportunity cost of training resources which could otherwise be allocated to other activities.

[60] That is, instead of including as costs $(1 + \epsilon + \Omega)$ multiplied by each annual UI value, UI could easily be incorporated into benefits which cancels out the "1". We thus only need to concern ourselves with dead-weight loss and distortion parameters.

values ranging between 0.25 and 0.75. I interpret this as a lower bound for at least two reasons. First, earnings returns are calculated from an intent-to-treat (ITT) estimator which likely understates benefits due to imperfect compliance with the treatment (i.e. partial take-up of TAA attenuates earnings estimates toward zero). Second, TAA may induce worker substitution away from other costly social insurance programs such as disability insurance (DI), which would further decrease program costs.[61]

One important caveat however, is that positive earnings returns are estimated from a local average treatment effect (LATE) that does not capture the fact that firms may be incentivized to lay off *additional* workers after learning of their TAA eligibility status. Nevertheless, back-of-the-envelope calculations show that these are unlikely to outweigh potentially much larger downward pressures. Finally, it bears noting that like other public programs, the additional public revenue from taxation of increased earnings returns for TAA takers is not sufficient to cover program costs.

7 Concluding Remarks

One of the most prominent challenges facing low-income workers in a modern, global labor market, is how labor will adjust to a rapidly changing and increasingly automated economy. Trade-impacted workers have received a large share of this attention, which became particularly visible as a policy issue during the 2016 Presidential Election. While low-income households have benefited tremendously from trade in terms of lower costs of goods, higher variety, and quality of life improvements associated with technological advancement, recent evidence suggests that they have also been persistently negatively affected in terms of earnings and employment outcomes. Despite growing evidence that trade's disperse benefits also come with concentrated costs, little is known about policy efforts that deliberately target the adjustment process for those most affected by regionally and industrially shocked labor markets. Credible empirical estimates are made difficult by both a lack of detailed worker-level data and confounding factors correlated with qualifying for adjustment programs—selection biases which generally preclude reliable estimates.

This paper estimates the causal effects of Trade Adjustment Assistance (TAA)—the United States' largest and longest standing public incentive program for retraining—on worker outcomes, by leveraging quasi-random assignment of TAA cases to investigators of varying approval leniencies. Using employer-employee matched Census data on 300,000 displaced workers, the paper provides evidence of large initial returns to TAA. Workers inferred to take up benefits forego roughly $10,000 in income while training, yet ten years later have approximately $50,000 higher cumulative earnings relative to all-else-equal workers that do not retrain. I estimate that 40% of these returns are driven by higher

[61] Autor et al. (2014) find that DI is in fact the predominant margin through which workers adjust to trade shocks.

wages—a sizable share which suggests that TAA-trained workers are not only compensated through greater labor force participation or higher priority in job queues. Rather, TAA workers also appear to be paid a premium for their newly acquired human capital.

But these large relative gains also *decay* over time. In fact, annual incomes among TAA and non-TAA workers fully converge after ten years. In conjunction with two additional pieces of evidence—that TAA has no effect on formal education, and diminishing returns are restricted to states with low training durations—I attribute this depreciation to short-run demanded skills becoming obsolete (consistent with rapid skill-biased technological change or an overall declining labor share). Indeed, 62% of TAA training programs confer vocational degrees with shorter program lengths than typical community college or 4-year college degrees which have been shown to have durable earnings returns.

I provide suggestive evidence that TAA-trained workers were previously bound by adjustment frictions. However, one limitation to the study is that I do not identify whether TAA is effectively expanding a feasible job match radius for its participants, or instead alleviating other frictions, which may include worker present-bias, household liquidity constraints, and other hypotheses. If in fact these workers are facing trade shocks which are spatially correlated (and industrially concentrated if sufficiently agglomerated in those regions), this might also explain why earnings returns are especially strong among movers.

Lastly, a cost-benefit analysis produces a conservative internal rate of return (IRR) on TAA between 0.0% and 9.1%. Despite this being a relatively low IRR, policymakers may still consider the efficiency costs associated with TAA investments a worthwhile trade-off as a redistributive policy toward this sub-population. This would be especially true if there were either other externalities associated with TAA training, or current social insurance programs were shown to be insufficient for these workers.

One outstanding and related puzzle is if labor markets signal high private returns to human capital investments such as TAA training, and this study confirms those high returns, then why are take-up rates so persistently low? Future work will need to explain this puzzle, and further examine whether the results from this paper are relevant to other types of labor market pressures such as automation.

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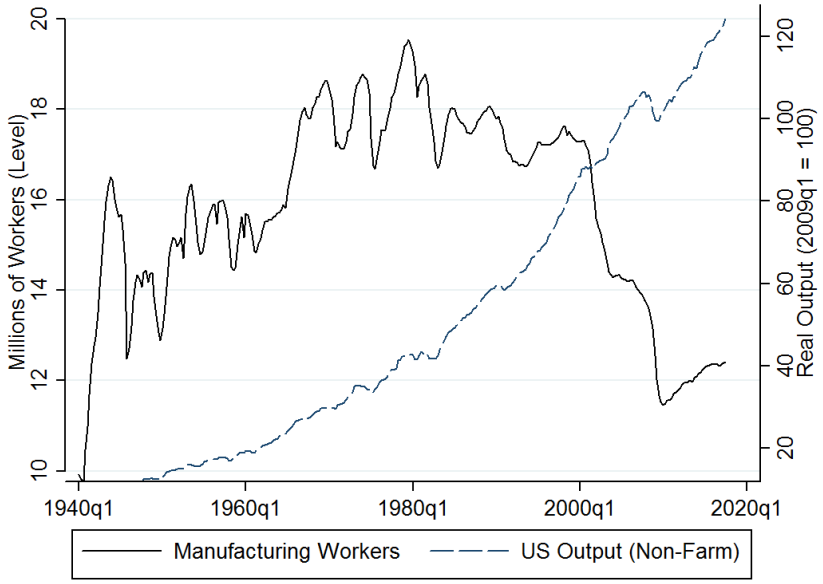
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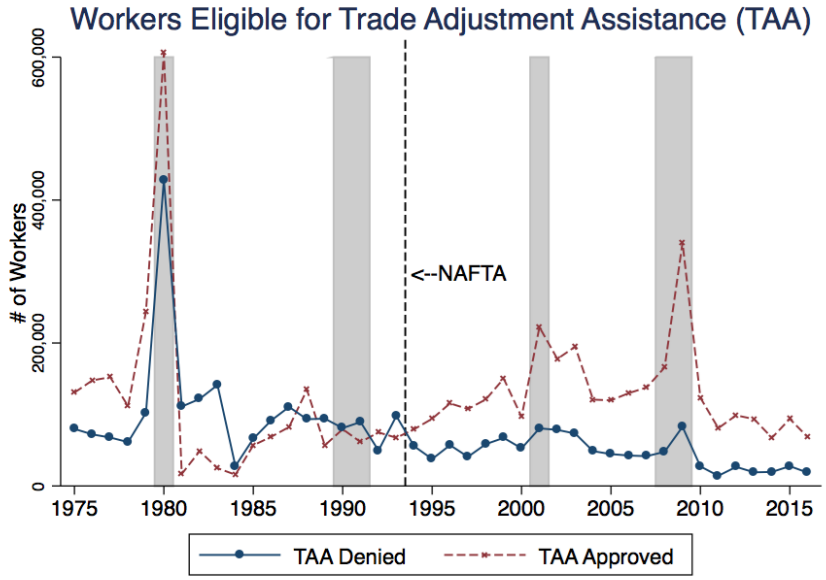
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Figure 1. Manufacturing Employment and Real Output



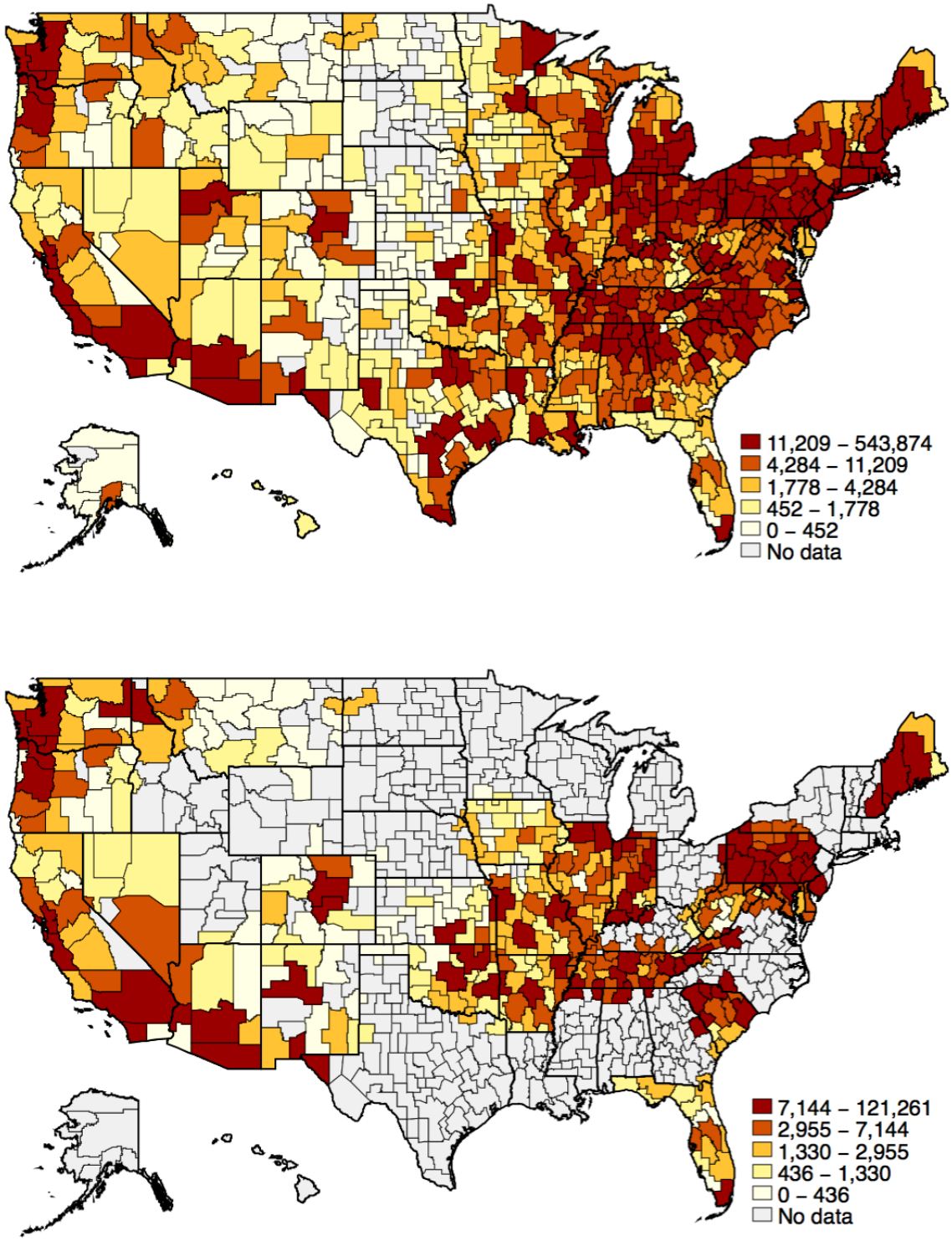
NOTES—This figure shows seasonally adjusted data from the Bureau of Labor Statistics (BLS), retrieved from FRED, Federal Reserve Bank of St. Louis. The solid line plots monthly US workers employed in manufacturing industries (BLS Current Employment Statistics, CES3000000001). The dashed line shows a quarterly index of nonfarm business sector real output (BLS Labor productivity and Costs) where 2009q1=100. In a similar figure, Pierce and Schott (2016) show the upward pattern in output persists for manufacturing sector value added, which suggests falling labor intensity drives the decline rather than secular stagnation. Source: BLS (2017)

Figure 2. TAA Filer Time Series (DOL Estimates)



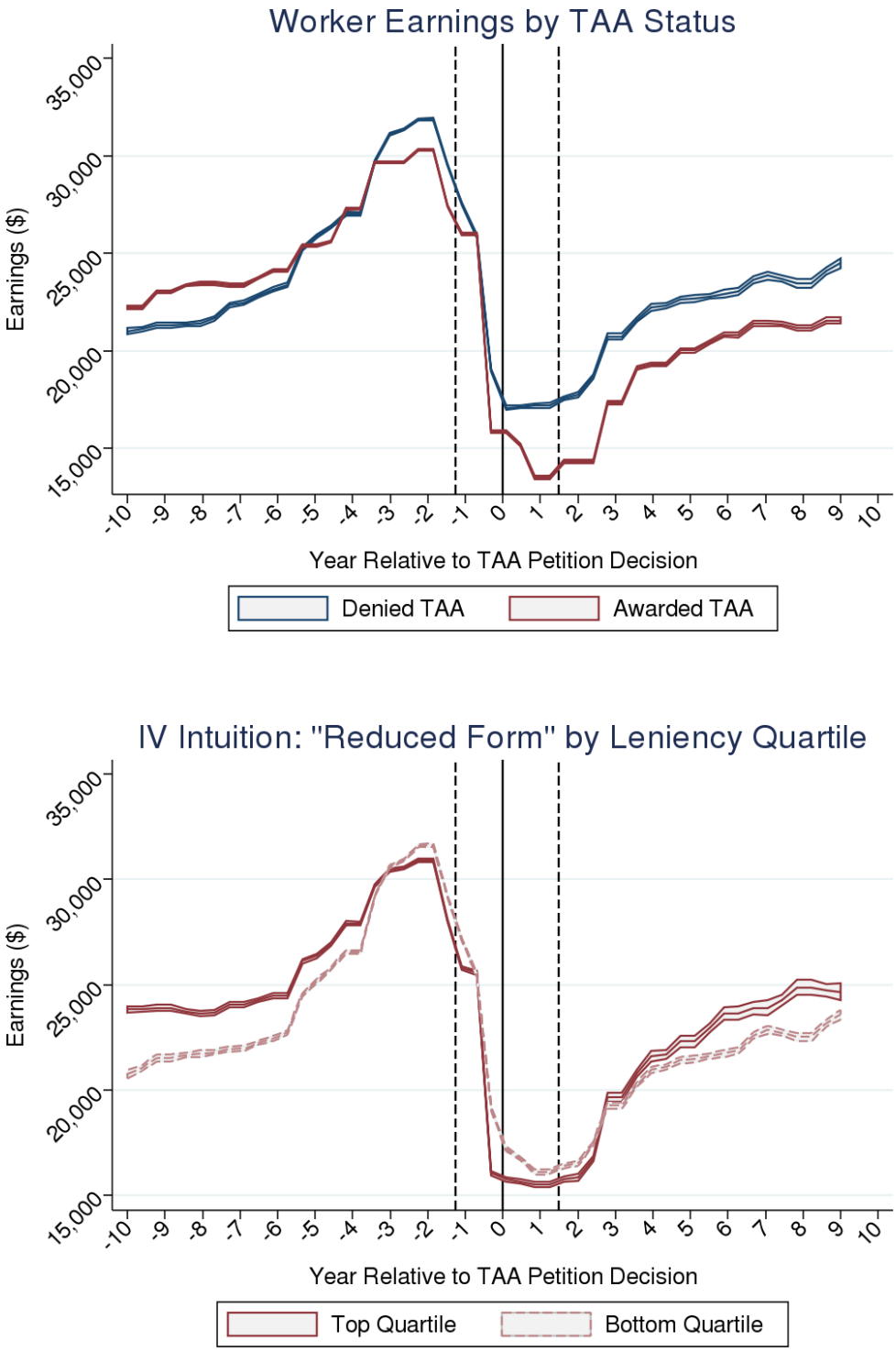
NOTES—This figure shows the estimated number of workers eligible for TAA benefits as covered by petitions at the Department of Labor. Worker estimates are generated by case investigators processing plant-level information (at times acquired using subpoena power), and cover the entire 3-year eligibility window shown in the timeline in Section 2. Recession quarters are indicated in gray, and taken from NBER Business Cycle Dating Committee data. The final matched analysis sample studies petitions filed between 1990 and 2011 for a subset of 23 approved states (see text for further details). Source: DOL (OTAA) petition database attained via FOIA request; NBER Business Cycle Dating Committee

Figure 3. Workers Filing for TAA: All (Top) versus LEHD Sample (Bottom)



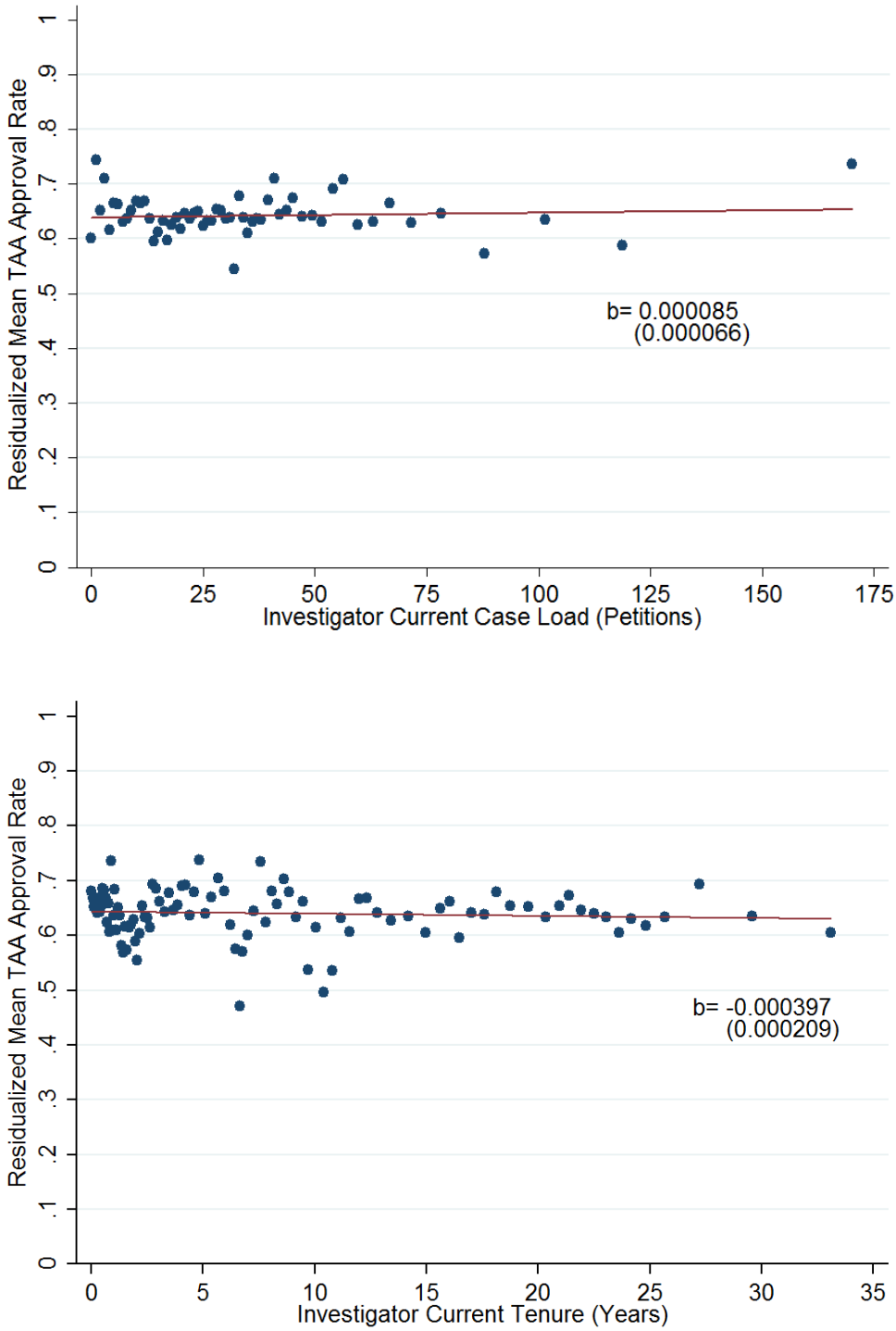
NOTES—These maps show the cumulative number of TAA filers by 1990 commuting zone (geographies taken from Dorn (2009)). The top sample reflects the universe of TAA filers from 1974 to 2016. The bottom sample displays all filers in 24 LEHD-approved states from 1990 to 2011, forming the basis of the matched analysis sample. Both maps display cumulative numbers by quintile. See Appendix B.6 for a population-weighted version of the same maps. Source: DOL (OTAA) petition database attained via FOIA request

Figure 4. Displaced Worker Earnings, naive Difference-in-Differences (Top) versus Leniency IV "Reduced Form" (Bottom)



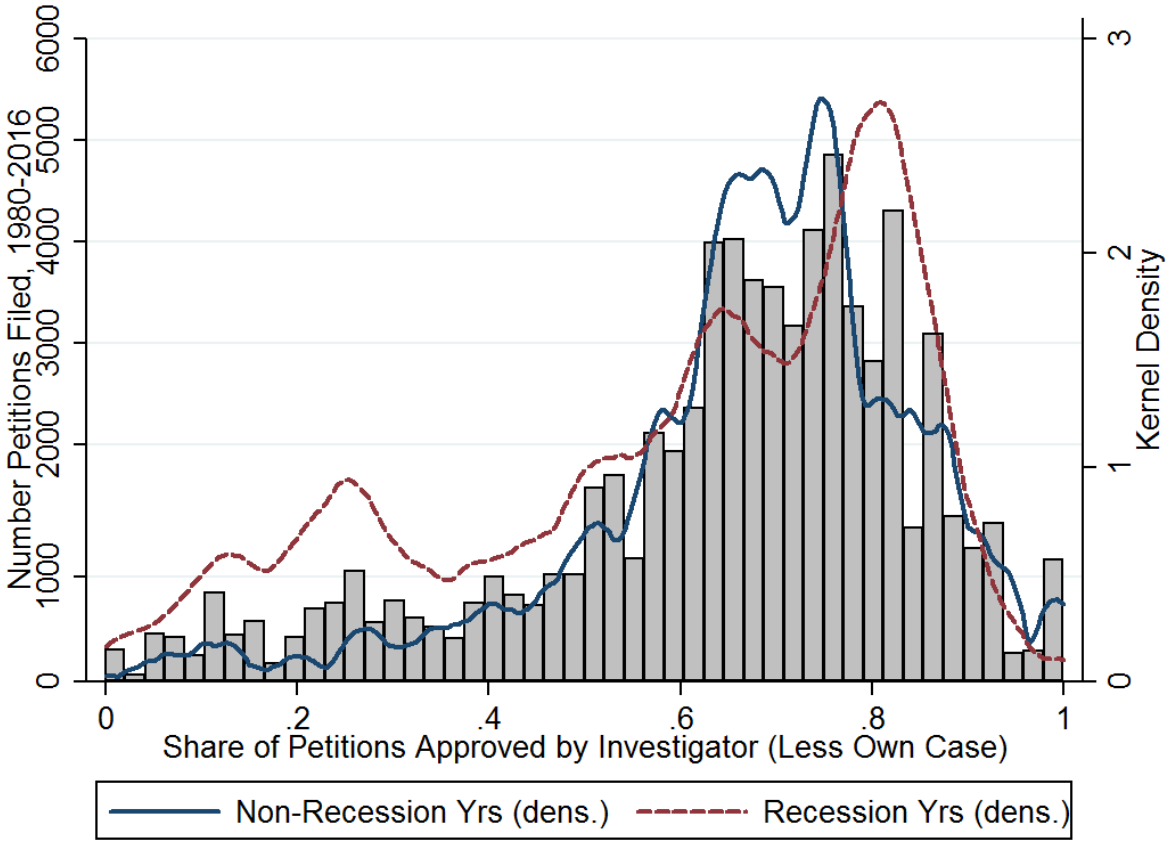
NOTES—These plots show locally smoothed polynomial regressions of annual earnings before and after worker separation (with 10% confidence intervals for exposition). Unconditional means are estimated for the main analysis sample: working-age individuals (22-65) with 2 years of positive earnings above the annual minimum wage equivalent prior to filing ($7.25 * 2,082 average working hours in 2010 ≈ $15,000), earning less than $50,000 annually in the pre-period. Monetary variables are deflated to 2010 US dollars, seasonally adjusted, and winsorized at 1% to limit outliers. Leniency quartiles (bottom panel) are calculated across 250 investigators. 25th and 75th percentiles correspond to approval rates of 0.56 and 0.79 respectively. Source: LEHD; DOL

Figure 5. Leniency IV versus Investigator Caseload and Tenure



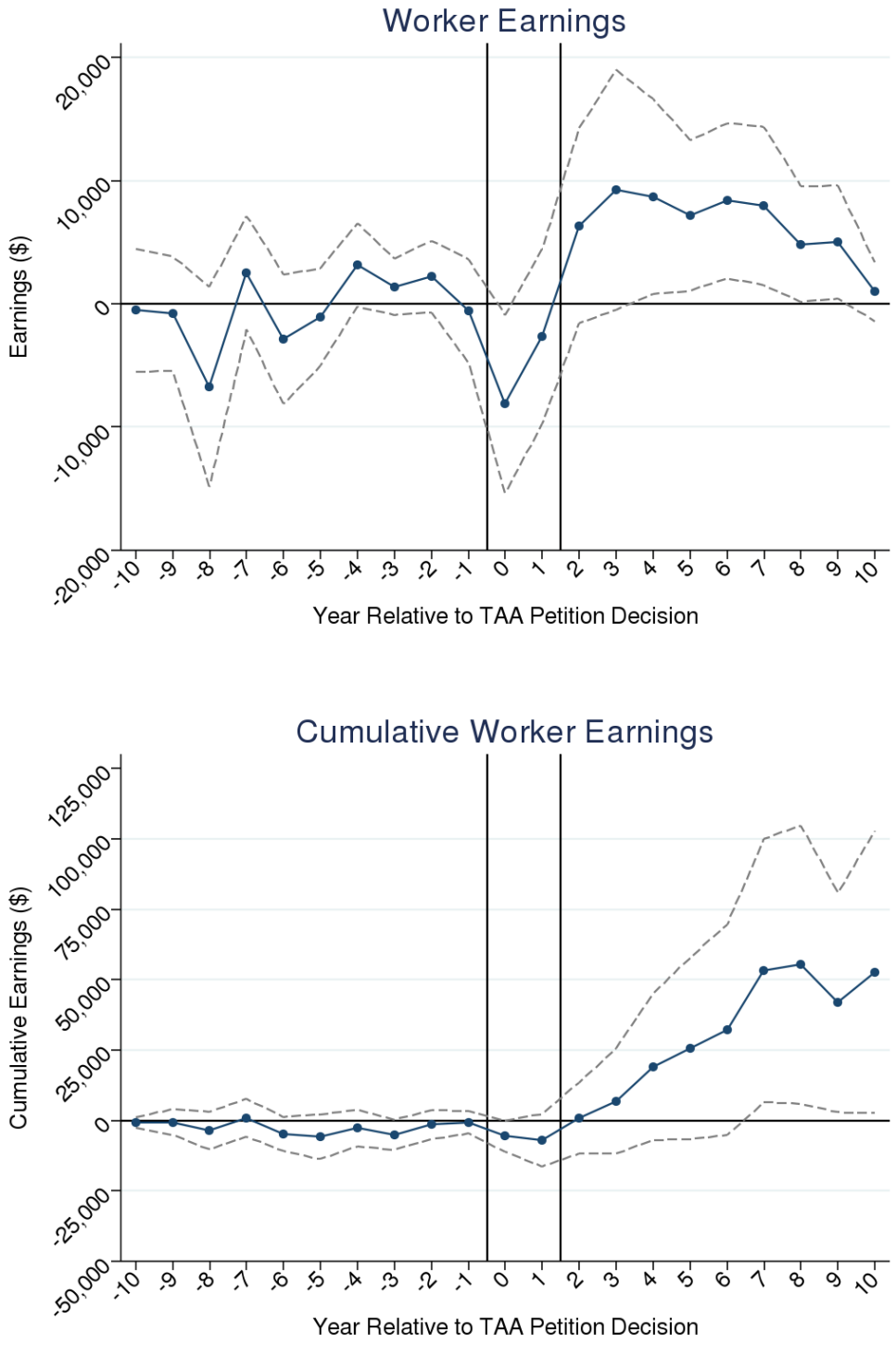
NOTES—These plots show the relationship between the main leave-one-out investigator leniency measure and: (1) investigator caseload measured by number of petitions (top); (2) investigator tenure in total years (bottom). Leniency is first regressed on year-of-filing fixed effects. Residuals from these regressions are then averaged across 100 quantile bins and plotted across the support of the x-axis variable. Residualized values are added to the overall mean leniency rate for ease of interpretation. Source: DOL historical employee records acquired via FOIA request

Figure 6. Variation in TAA Investigator Leniency



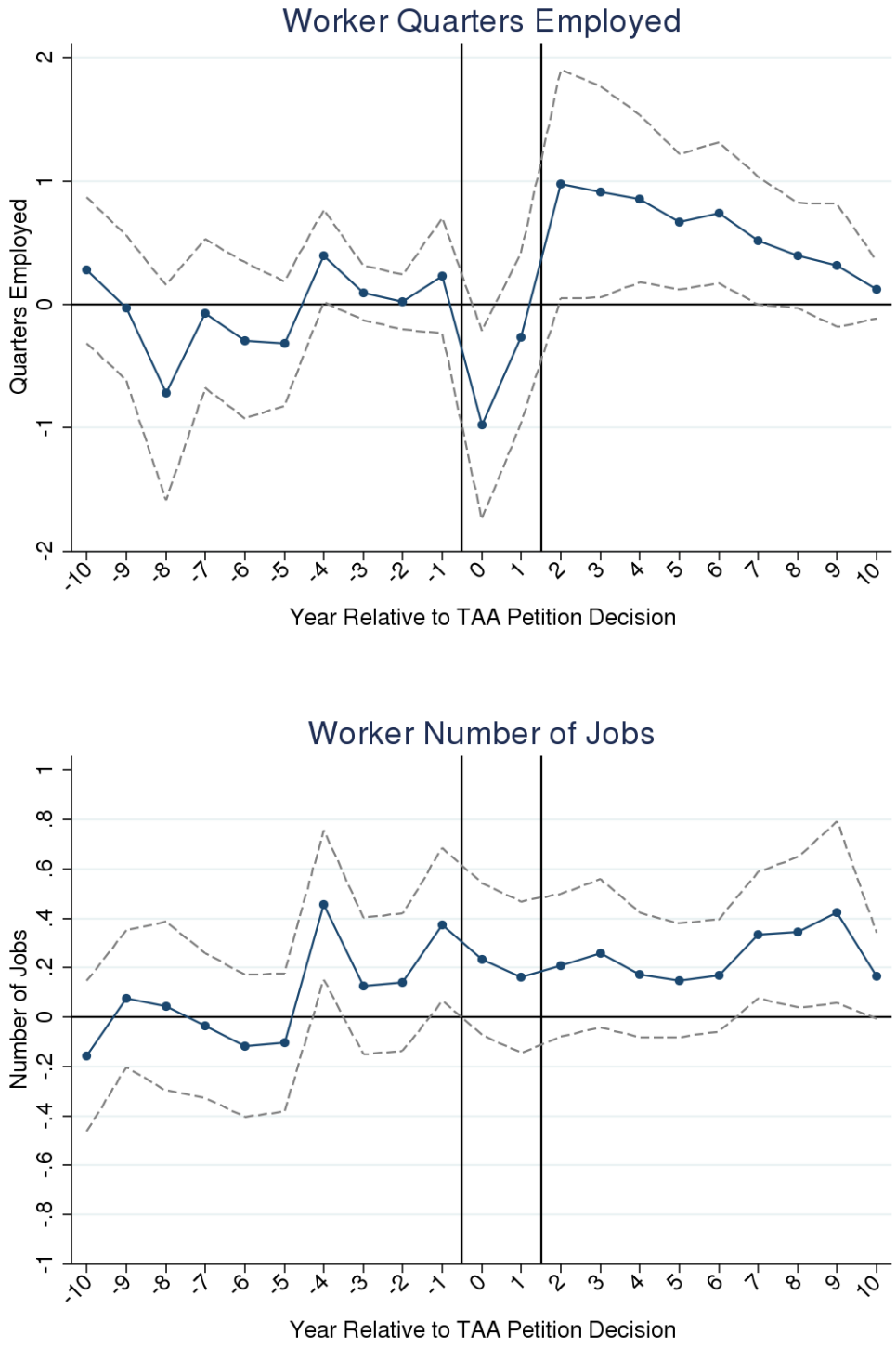
NOTES—This figure plots the number of petitions filed across 50 bins of investigator leniency (leave-one-out), pooled over the history of the TAA program (1974 to 2016). These are then overlaid with two kernel densities to express how the distribution of leniency varies across the business cycle. An analogous figure using the subset of 250 investigators associated with the final analysis sample is shown in the online appendix. Source: DOL historical employee records acquired via FOIA request

Figure 7. Dynamic Effects of TAA on Worker Earnings



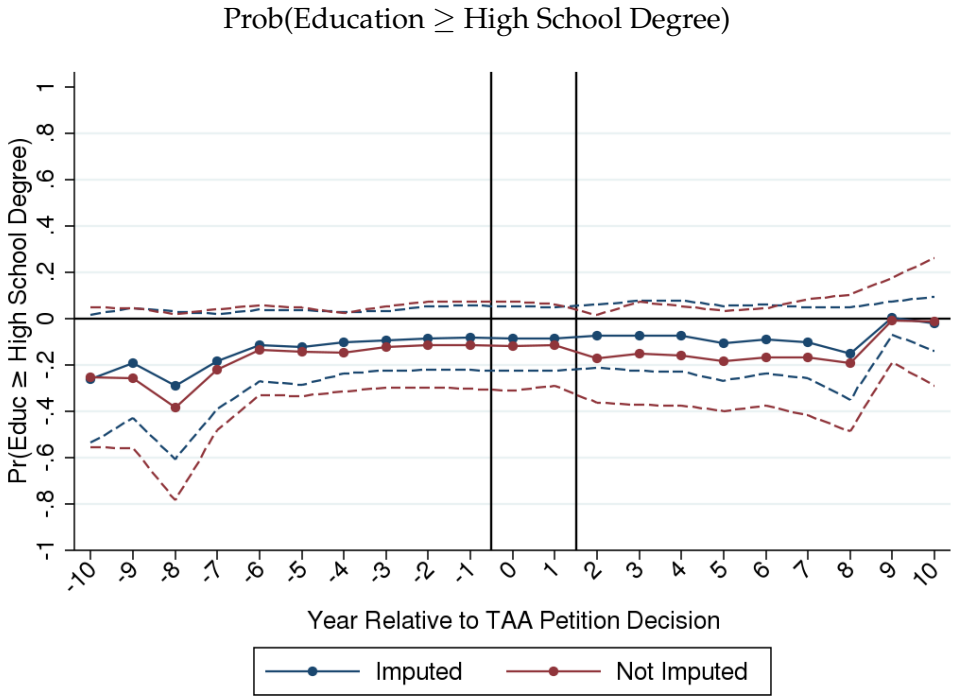
NOTES—These plots show the main 2SLS coefficient estimates of the effects of TAA on annual earnings (top) and within-worker cumulative earnings (bottom), dynamically across event years relative to the TAA petition decision ($\tau = 0$). Each point estimate is from a separate cross-sectional regression by event year. Cumulative earnings are summed from $\tau = -10$ to the event year on the x-axis. Vertical lines partition the support into pre-TAA, during-TAA, and post-TAA periods. Sample restrictions and variable definitions are identical to those described in Table 5, column 6 (the paper's preferred earnings specification). Dotted lines represent 90% confidence intervals with standard errors clustered at the investigator level. Source: LEHD; DOL

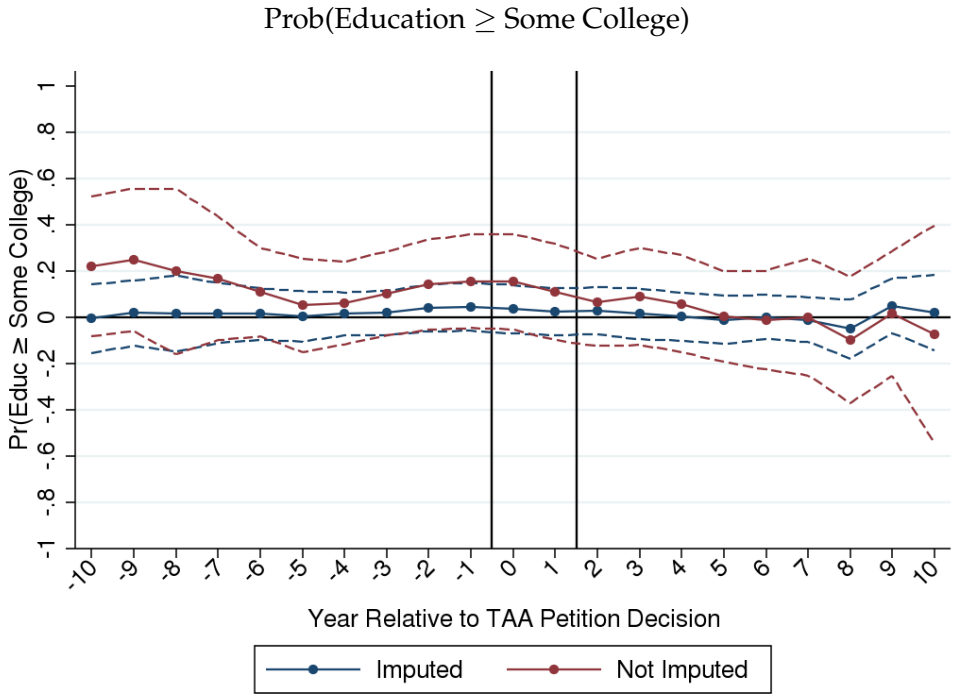
Figure 8. Dynamic Effects of TAA on Labor Force Participation



NOTES—These plots show the main 2SLS coefficient estimates of the effects of TAA on quarters employed (top) and number of jobs held (bottom), dynamically across event years relative to the TAA petition decision ($\tau = 0$). Each point estimate is from a separate cross-sectional regression by event year. Baseline quarters employed and number of jobs held are 3.56 and 1.11 respectively (Table 3, column 1). Vertical lines partition the support into pre-TAA, during-TAA, and post-TAA periods. Sample restrictions and variable definitions are identical to those described in Table 6, column 6 (the paper's preferred employment specification). Dotted lines represent 90% confidence intervals with standard errors clustered at the investigator level. Source: LEHD; DOL

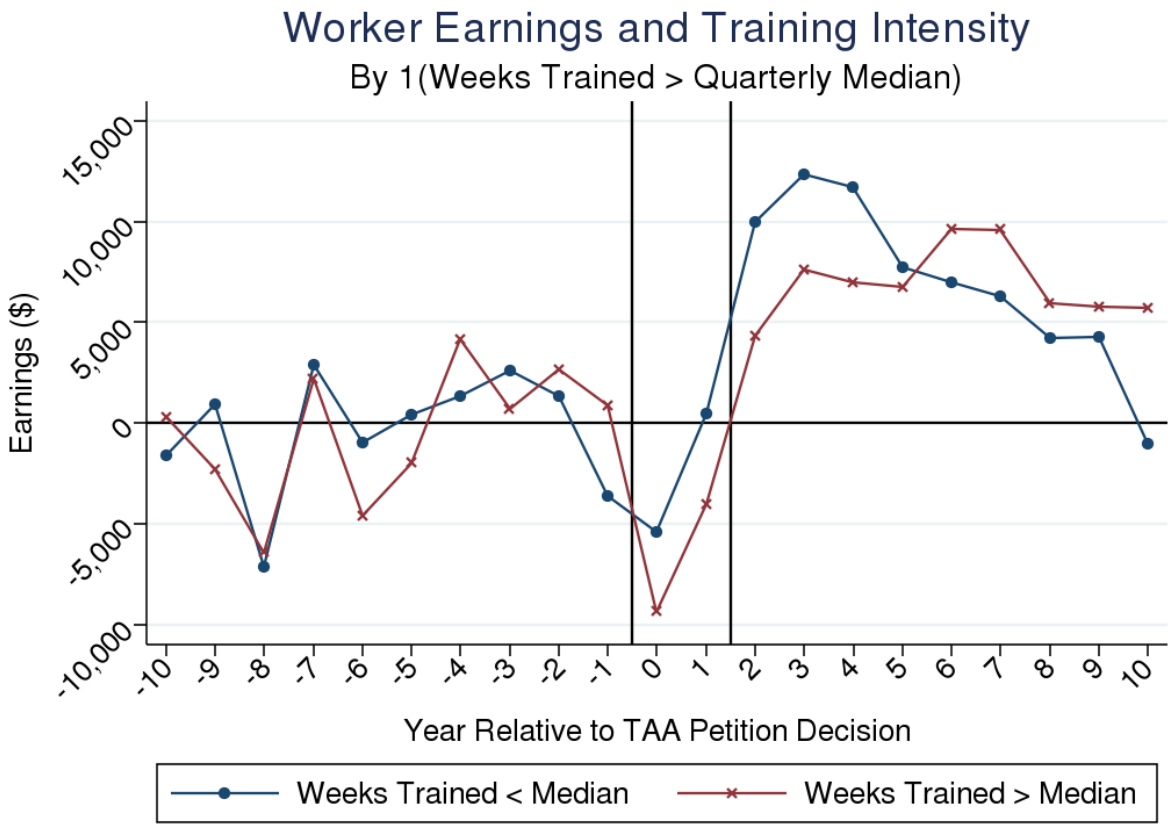
Figure 9. Dynamic Effects of TAA on Formal Education





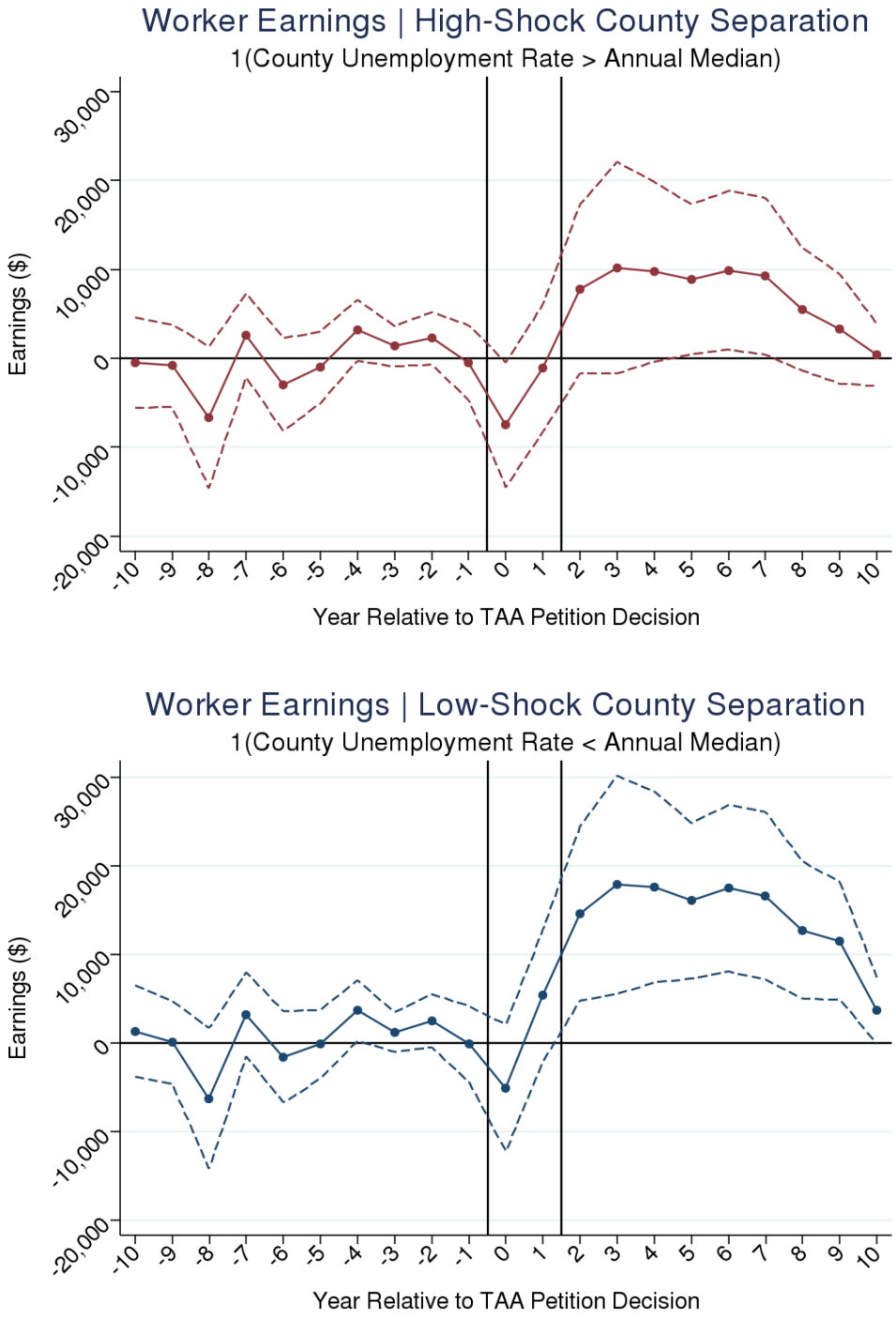
NOTES—These plots show the main 2SLS coefficient estimates of the effects of TAA on formal education take-up using both imputed and non-imputed decennial census measures which are merged to the Census LEHD (see text for details), dynamically across all event years relative to the TAA petition decision ($\tau = 0$). Each point estimate is from a separate cross-sectional regression by event year. Education can take four ordinal values: 1=Some High School; 2=High School Degree; 3=Some College; 4=College Degree. Vertical lines partition the pre-TAA, during-TAA, and post-TAA periods. Sample restrictions and variable definitions are identical to those described in Table 7, column 5 (the paper's preferred education specification). Dotted lines represent 90% confidence intervals with standard errors clustered at the investigator level. Source: LEHD; DOL

Figure 10. Heterogeneous Earnings Effects of TAA by State Training Duration



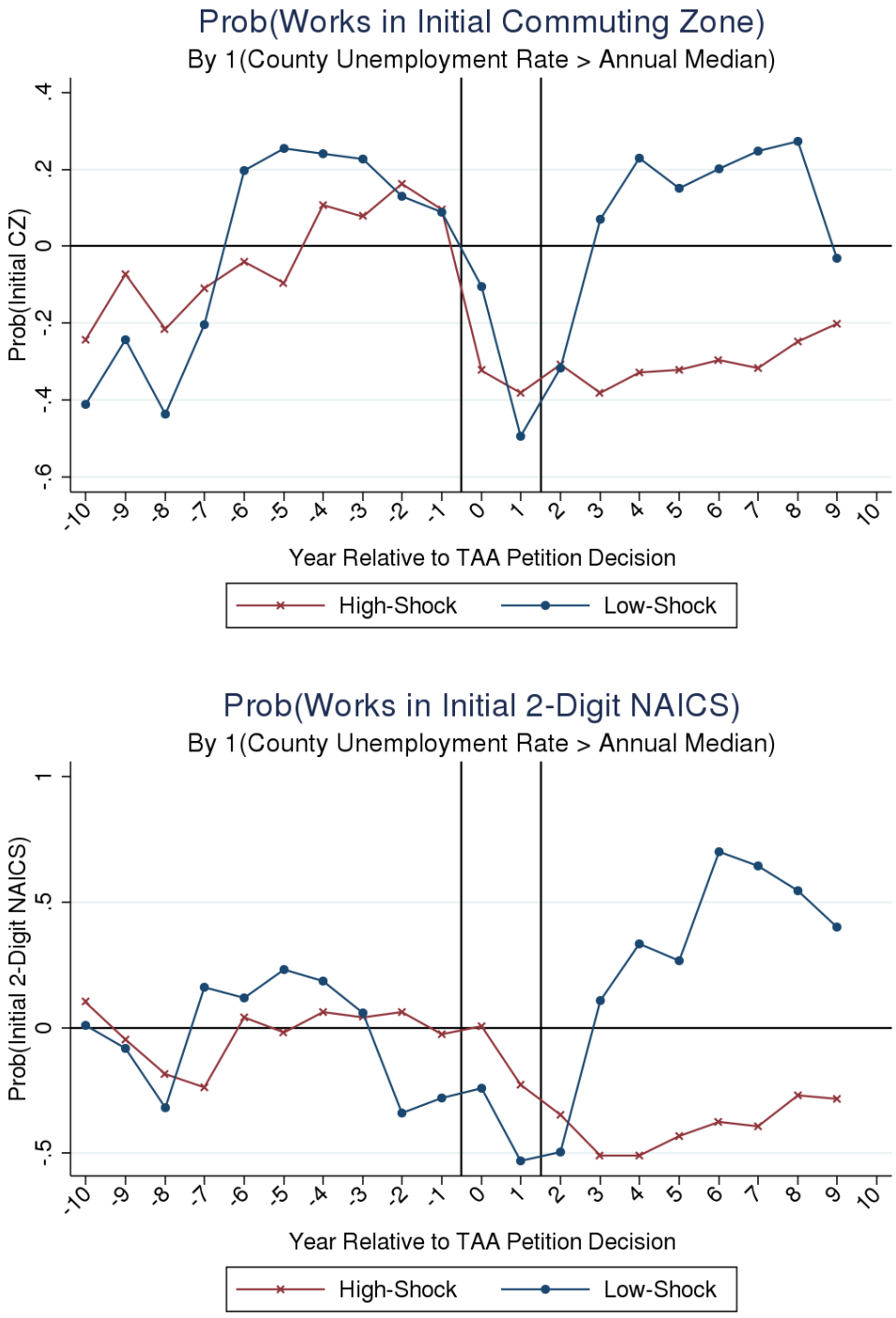
NOTES—This figure overlays two series of 2SLS estimates, corresponding to each $\beta_{1\tau}$ and $\beta_{2\tau}$ of equation (5) of the draft where coefficients reflect high and low training duration lengths. Each point estimate is from a separate regression by event year, showing the effects of TAA on worker earnings interacted with high and low training duration in the state and quarter in which the worker was laid off (pre-period). "High" and "Low" are defined relative to median training duration, which is calculated for each quarter across states using TAA performance data from 2001 to 2016 (training duration data are not available prior to 2001). Vertical lines partition the pre-TAA, during-TAA, and post-TAA periods. Besides the sub-sample from data limitations, remaining sample restrictions and variable definitions are identical to those described in Table 6, column 5. Standard errors are suppressed for exposition (see online appendix for underlying data and standard errors). Source: LEHD; DOL Trade Act Participant Report (TAPR), 2001 to 2016

Figure 11. Heterogeneous Earnings Effects of TAA by Labor Market Strength



NOTES—This figure shows two series of 2SLS estimates, corresponding to each $\beta_{1\tau}$ and $\beta_{2\tau}$ of equation (5) of the draft. Coefficients reflect workers displaced in high (top) and low (bottom) initial unemployment rate regions. Each point estimate is from a separate regression by event year, showing the effects of TAA on worker earnings by high and low training duration in the state and year in which the worker was laid off. "High" and "Low" are defined relative to median training duration, which is calculated for each quarter across states using TAA performance data from 2001 to 2016 (training duration data are not available prior to 2001). Vertical lines partition the pre-TAA, during-TAA, and post-TAA periods. Besides the data constraint from limited performance data, sample restrictions and variable definitions are identical to those described in Table 5, column 6. Dotted lines represent 90% confidence intervals with standard errors clustered at the investigator level. Source: LEHD; BLS Local Area Unemployment Statistics (LAUS)

Figure 12. Effects of TAA on Worker Mobility by Initial Labor Market Strength



NOTES—This figure shows two series of 2SLS estimates, corresponding to each $\beta_{1\tau}$ and $\beta_{2\tau}$ of equation (5) of the draft where the dependent variables are the probability that a worker is employed in the same commuting zone (top) and 2-digit NAICS industry (bottom) as the region and industry from which they initially separated. Estimates are broken out by high and low shock unemployment regions, where each point estimate is from a separate regression by event year. Commuting zones are defined by 1990 boundaries originally geocoded by Dorn (2009). Baseline mobility rates can be found in Table 3, column 1. Vertical lines partition the pre-TAA, during-TAA, and post-TAA periods. Sample restrictions and variable definitions are identical to those described in Table 6, column 6. Standard errors are suppressed for exposition. See online appendix for underlying data, and Figure 13 for corresponding earnings responses by mobility groups. Source: LEHD; BLS Local Area Unemployment Statistics (LAUS)

Figure 13. Earnings Effects of TAA for Labor Market and Industry "Movers"



NOTES—This figure shows 2SLS estimates of the effects of TAA for workers who move commuting zones (top) or switch 2-digit NAICS industries (bottom) in the post-period, relative to the region and industry in which they were initially employed. This *ex-post* definition of mobility is used as a subgroup in a specification similar to equation (5) of the draft, where only coefficients on movers are plotted here (see Appendix B.5 for analogous results for "stayers"). Movers comprise approximately 47% and 56% of the sample for commuting zones and industries respectively. Each point estimate is from a separate regression by event year. Vertical lines partition the pre-TAA, during-TAA, and post-TAA periods. Sample restrictions and variable definitions are identical to those described in Table 5, column 6. Dotted lines represent 90% confidence intervals with standard errors clustered at the investigator level. Source: LEHD; DOL

Table 1. Location of Top 10 "Trade Displaced" TAA Zip Codes

Rank	1975-1985	1985-1995	1995-2005	2005-2016
1.	Flint, MI	Seattle, WA	Los Angeles, CA	Fremont, CA
2.	Detroit, MI (a)	Fort Worth, TX	Seattle, WA	Detroit, MI
3.	Detroit, MI (b)	Houston, TX	East Chicago, IN	Nashville, TN
4.	Detroit, MI (c)	Linden, NJ	Long Beach, CA	St. Louis, MI
5.	Detroit, MI (d)	Flint, MI	San Jose, CA	Wichita, KS
6.	Detroit, MI (e)	Los Angeles, CA	Scranton, PA	Dayton, OH
7.	Saginaw, MI	Houston, TX	Washington, NC	Warren, OH
8.	Dayton, OH	Milwaukee, WI	New York, NY	Wilmington, OH
9.	Kokomo, IN	Toledo, OH	Dallas, TX	Los Angeles, CA
10.	Ann Arbor, MI	Framingham, MA	Seattle, WA	Detroit, MI
Affected Industries (Mode)	Motor Vehicles, Parts, Accessories	Oil & Gas Extract., Exploration, Services	Electronics, Aircraft, Textiles	Auto Parts, Plastics Aircraft, Computers Pharmaceuticals

NOTES—This table reports the locations of the top ten "trade-displaced" zip codes as calculated from Department of Labor estimates of the total number of workers eligible for TAA, as associated with filed TAA petitions at the plant level. The reported industry reflects the qualitative description associated with the top three to five modal industries in the same decade using standard industrial classification (SIC) codes ascribed to petitions by case investigators. Source: DOL (OTAA) petition database attained via FOIA request

	High Labor Force Attachment						High & Low Labor Force Attachment					
	TAA Approved		TAA Denied		All		TAA Approved		TAA Denied		All	
	(1)		(2)		(3)		(1)		(2)		(3)	
A. Worker Demographics (LEHD)												
Age (time of filing)	43.91	(10.93)	43.08	(11.09)	43.65	(10.99)	42.7	(11.13)	41.76	(11.16)	42.41	(11.15)
Female	0.54	(0.50)	0.50	(0.50)	0.53	(0.50)	0.56	(0.50)	0.52	(0.50)	0.55	(0.50)
Black	0.11	(0.32)	0.13	(0.33)	0.12	(0.32)	0.13	(0.33)	0.15	(0.35)	0.13	(0.34)
White	0.80	(0.40)	0.79	(0.40)	0.80	(0.40)	0.79	(0.41)	0.78	(0.42)	0.78	(0.41)
High School Degree	0.39	(0.49)	0.37	(0.48)	0.39	(0.49)	0.38	(0.49)	0.37	(0.48)	0.37	(0.48)
Some College	0.29	(0.45)	0.31	(0.46)	0.30	(0.46)	0.29	(0.45)	0.31	(0.46)	0.29	(0.45)
College Degree and Above	0.11	(0.31)	0.14	(0.35)	0.12	(0.33)	0.12	(0.32)	0.14	(0.35)	0.13	(0.33)
B. Worker Economic Variables (LEHD)												
Annual Earnings ($1,000)	22.554	(13.511)	23.468	(14.793)	22.83	(13.916)	18.628	(13.904)	19.232	(15.136)	18.814	(14.294)
Quarters Employed	3.21	(1.28)	3.14	(1.33)	3.19	(1.30)	2.93	(1.42)	2.87	(1.47)	2.92	(1.44)
Quarters Employed (full-time equivalence)	2.77	(1.54)	2.71	(1.57)	2.75	(1.55)	2.28	(1.67)	2.22	(1.70)	2.26	(1.68)
Employed	0.79	(0.40)	0.76	(0.43)	0.78	(0.41)	0.74	(0.44)	0.7	(0.46)	0.73	(0.45)
# of Jobs / Year	0.95	(0.75)	0.92	(0.78)	0.94	(0.76)	0.94	(0.08)	0.91	(0.85)	0.93	(0.84)
Tenure (years at separation)	4.90	(3.71)	3.87	(3.24)	4.58	(3.60)	14.76	(14.52)	11.5	(12.29)	13.74	(13.94)
Init. Employer Mean Earnings ($1,000)	23.197	(5.628)	23.216	(5.596)	23.204	(5.617)	20.822	(6.59)	21.100	(6.338)	20.910	(6.513)
County Unemployment Rate (time of filing)	6.51	(2.28)	6.4	(1.85)	6.48	(2.16)	6.43	(2.20)	6.40	(1.78)	6.42	(2.08)
Prob(Employed at Petitioner Firm)	0.66	(0.47)	0.66	(0.47)	0.66	(0.47)	0.63	(0.48)	0.64	(0.48)	0.63	(0.48)
Prob(Employed at Petitioner 2-Digit NAICS)	0.7	(0.46)	0.71	(0.45)	0.70	(0.46)	0.68	(0.47)	0.69	(0.46)	0.68	(0.47)
Prob(Employed in Petitioner Commuting Zone)	0.69	(0.46)	0.72	(0.45)	0.70	(0.46)	0.67	(0.47)	0.72	(0.45)	0.69	(0.46)
C. Petitioner Characteristics (DOL)												
Company Filer	0.40	(0.49)	0.10	(0.3)	0.31	(0.46)	0.40	(0.49)	0.11	(0.31)	0.31	(0.46)
Union Filer	0.14	(0.34)	0.19	(0.39)	0.15	(0.36)	0.13	(0.34)	0.17	(0.38)	0.14	(0.35)
Worker-Group Filer	0.37	(0.48)	0.63	(0.48)	0.45	(0.50)	0.37	(0.48)	0.65	(0.48)	0.46	(0.50)
Investigator Caseload (no. of petitions)	22.83	(18.25)	22.27	(16.21)	22.66	(17.64)	23.05	(18.68)	22.44	(16.35)	22.86	(17.99)
Investigator Tenure (decades)	0.979	(0.942)	1.02	(0.879)	0.991	(0.925)	1.02	(0.950)	0.995	(0.872)	1.01	(0.928)
Prob(Tradable) (Mian & Sufi, 2011)	0.64	(0.48)	0.51	(0.50)	0.60	(0.49)	0.58	(0.49)	0.43	(0.50)	0.53	(0.50)
Offshorability Z-Score (Blinder, 2007)	0.29	(0.84)	0.09	(1.23)	0.22	(0.98)	0.25	(0.87)	0.02	(1.28)	0.18	(1.02)
Offshorability Z-Score (Autor & Dorn, 2013)	0.23	(0.86)	0.27	(0.79)	0.24	(0.84)	0.16	(0.84)	0.22	(0.78)	0.17	(0.82)
Number of Petitioners	∼3,100		∼1,300		∼4,300		∼3,300		∼1,400		∼4,700	
Number of Displaced Workers	∼123,000		∼54,000		∼177,000		∼198,000		∼89,000		∼287,000	
Number of Observations	∼1,811,000		∼810,000		∼2,623,000		∼2,883,000		∼1,307,000		∼4,192,000	

NOTES—This table reports means and standard deviations for approved and denied TAA petitioners, pooled across all periods (pre, during, *and* post-TAA). Both samples are restricted to working-age individuals (22-65) making less than $50,000 annually in the pre-period, and restricted to first-time filers. The "High Attachment" sample requires 2 years of positive earnings above the annual minimum wage equivalent prior to filing ($7.25 * 2,082 average working hours in 2010 ≈ $15,000). Monetary variables are deflated to 2010 US dollars, seasonally adjusted, and winsorized at the 1% level to limit the influence of outliers. Both samples contain 250 unique case investigators (rounded for confidentiality). See text for variable descriptions. Source: LEHD, DOL

Table 3. Testing for Random Assignment of TAA Petitions to Investigators

	Baseline	TAA Approved		Investigator Leniency						F-Test [p-value]
	(1)	(2)		(3)		(4)		(5)		(6)
A. Pre-Determined Demographics (LEHD)										
Age (time of filing)	40.771	0.0007*	(0.0004)	-0.00007	(0.0001)	-0.00002	(0.00004)	-0.00002	(0.00004)	[0.4437]
Female	0.5248	0.0281**	(0.0118)	0.0015	(0.0019)	-0.0005	(0.0009)	-0.0006	(0.0009)	[0.5166]
Black	0.1147	-0.0415*	(0.0229)	-0.0081**	(0.0033)	-0.0035	(0.0026)	-0.0034	(0.0025)	[0.3025]
White	0.7957	-0.0192	(0.0151)	-0.0018	(0.0025)	-0.0001	-0.0020	-0.0001	(0.0020)	[0.2819]
High School Degree	0.3865	-0.0057	(0.0069)	-0.0020	(0.0015)	-0.0011	(0.0012)	-0.0010	(0.0011)	[0.8490]
Some College	0.2936	-0.0110	(0.0068)	-0.00189	(0.0015)	-0.0007	(0.0011)	-0.0006	(0.0010)	[0.3707]
College Degree	0.1173	-0.0224**	(0.0089)	-0.0014	(0.0021)	-0.0013	(0.0011)	-0.0012	(0.0011)	[0.3110]
B. Pre-Determined Economic Variables (LEHD)										
Annual Earnings ($1,000)	26.880	-0.0021***	(0.0006)	-0.0002	(0.0001)	-0.0001	(0.00007)	-0.00007	(0.00007)	[0.0917]
Quarters Employed	3.5566	-0.0060	(0.0048)	0.0011	(0.0012)	0.0007	(0.0008)	0.0006	(0.0008)	[0.0627]
Quarters Employed (full-time equivalence)	3.2330	0.0154***	(0.0041)	0.0011	(0.0009)	0.0002	(0.0004)	0.0002	(0.0005)	[0.0586]
Employed	0.9236	0.0420**	(0.0189)	-0.0040	(0.0027)	-0.0025	(0.0025)	-0.0021	(0.0025)	[0.2100]
# of Jobs / Year	1.1120	-0.0007	(0.0008)	0.0002	(0.0002)	0.0002*	(0.0001)	0.0002**	(0.0001)	[0.2325]
Tenure (years at separation)	5.0390	0.0012***	(0.0004)	0.0002**	(0.0001)	0.0001**	(0.0001)	0.0001**	(0.0006)	[0.0291]
Init. Employer Mean Earnings ($1,000)	23.159	0.0008	(0.0024)	0.0006	(0.0004)	0.0005	(0.0004)	0.0005	(0.0004)	[0.1070]
County Unemployment Rate (time of filing)	6.6054	0.0070	(0.0059)	-0.0001	(0.0012)	0.0005	(0.0009)	0.0008	(0.0009)	[0.0952]
Prob(Employed at Petitioner Firm)	0.7409	-0.0114	(0.0171)	0.0005	(0.0022)	-0.0012	(0.0019)	-0.0012	(0.0019)	[0.0423]
Prob(Employed at Petitioner 2-Digit NAICS)	0.7760	0.0177	(0.0211)	-0.0004	(0.0029)	-0.0007	(0.002)	-0.0008	(0.0020)	[0.7783]
Prob(Employed in Petitioner Commuting Zone)	0.6853	-0.0245	(0.0253)	0.0026	(0.0046)	0.0067**	(0.0031)	0.0066**	(0.0031)	[0.4896]
C. Pre-Determined Petitioner Characteristics (DOL)										
Company Filer	0.3267	0.2190***	(0.0436)	0.0024	(0.0072)	0.0012	(0.0079)	0.0016	(0.0079)	[0.5251]
Union Filer	0.1415	-0.0068	(0.0491)	-0.0043	(0.0086)	-0.0038	(0.0085)	-0.0038	(0.0086)	[0.8211]
Worker-Group Filer	0.4354	-0.0643	(0.0469)	0.0001	(0.0070)	-0.0024	(0.0075)	-0.00152	(0.0074)	[0.8612]
Investigator Caseload (no. of petitions)	23.2509	0.00001	(0.0009)	-0.0003*	(0.0002)	-0.00008	(0.0002)	-0.00005	(0.0002)	[0.3175]
Investigator Tenure (decades)	0.9419	0.00002	(0.00003)	-0.00004**	(0.00002)	-0.00004**	(0.00002)	-0.00004**	(0.00002)	[0.0177]
Prob(Tradable) (Mian & Sufi, 2011)	0.6008	0.0687**	(0.0270)	0.0009	(0.0048)	–	–	–	–	[0.0952]
Offshorability Z-Score (Blinder, 2007)	0.3076	0.0032	(0.0176)	-0.0058**	(0.0025)	–	–	–	–	[0.0967]
Offshorability Z-Score (Autor & Dorn, 2013)	0.2533	-0.0121	(0.0103)	-0.0021	(0.0018)	–	–	–	–	[0.1812]
Filer Industry FEs (4-digit SIC)	–					yes		yes		yes
Investigator Concentration Controls	–							yes		yes
Joint F-Test [p-value]	–	[0.0000]		[0.0409]		[0.2033]		[0.1801]		[0.1801]
Number of Petitioners	–	∼4,300		∼4,300		∼4,300		∼4,300		∼4,300
Number of Employers	–	∼86,000		∼86,000		∼86,000		∼86,000		∼86,000
Number of Displaced Workers	–	∼177,000		∼177,000		∼177,000		∼177,000		∼177,000
Number of Observations	–	∼1,381,000		∼1,381,000		∼1,381,000		∼1,381,000		∼1,381,000

NOTES—This table tests for random assignment of TAA petitions to DOL investigators. Observations are pooled from eligible separating workers in the "High Attachment" sample 40 to 5 quarters before their petition decision, and restricted to first-time filers. The sample includes working-age individuals (22-65) making under $50,000 annually in the pre-period, with 2 years above the annual minimum wage equivalent prior to filing ($7.25 * 2,082 average working hours in 2010 $\approx$ $15,000). Column (1) reports the baseline mean. The dependent variable in column (2) is an indicator for TAA approval. The dependent variable in columns (3) to (5) is the investigator leniency IV. Column (6) shows p-values from separate regressions of the leniency IV on each covariate. Monetary variables are deflated to 2010 US dollars, seasonally adjusted, and winsorized at 1% to limit outlier influence. All regressions contain quarter-of-filing and calendar-year fixed effects. Standard errors are clustered at the investigator level. ***p<0.01, **p<0.05, *p<0.10. Source: LEHD, DOL

Table 4. TAA Approval and Investigator Leniency (First Stage)

	1(TAA) (1)	**1(TAA)** (2)	**1(TAA)** (3)	**1(TAA)** (4)	**1(TAA)** (5)
Leniency (Leave-One-Out)	0.646***	0.613***	0.639***	0.615***	0.592***
	(0.11)	(0.11)	(0.12)	(0.12)	(0.11)
Investigator # of Cases in Industry (N_{jkt})	0.00274***	0.00278***	0.00270***	0.00268***	0.00296***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Investigator Concentration in Industry (C_{jkt})	-0.524***	-0.499***	-0.508***	-0.481***	-0.411***
	(0.11)	(0.11)	(0.12)	(0.12)	(0.11)
Investigator Caseload (No. of Petitions)			-0.0001	-0.0002	-0.0003
			(0.00)	(0.00)	(0.00)
Investigator Tenure (Decades)			0.000003	0.000003	0.000004*
			(0.00)	(0.00)	(0.00)
Filer Industry FEs (4-Digit SIC)	yes	yes	yes	yes	yes
Calendar-Year & Filer-Quarter FEs		yes	yes	yes	yes
Baseline Controls				yes	yes
Full Regression Controls					yes
Number of Petitioners	∼4,300	∼4,300	∼4,300	∼4,300	∼4,300
Number of Displaced Workers	∼177,000	∼177,000	∼177,000	∼177,000	∼177,000
Number of Observations	∼2,623,000	∼2,623,000	∼2,623,000	2,623,000	∼2,623,000
F-Statistic on Excluded IV	32.76	29.07	31.04	27.63	27.21

NOTES—This table reports first stage estimates corresponding to equation (1) of the paper for the "High Attachment" sample, which includes working-age individuals (22-65) making under $50,000 annually in the pre-period, with 2 years above the annual minimum wage equivalent prior to filing ($7.25 * 2,082 average working hours in 2010 ≈ $15,000). *Baseline Controls* include means over 5 to 40 quarters prior to the TAA petition decision for the following variables: worker earnings, quarters employed, worker tenure, and initial employer and state earnings. *Full Regression Controls* reflect covariates estimated in the paper's preferred specification, and include: race and gender fixed effects, a cubic in age fully interacted with baseline earnings and gender indicators, filer-state fixed effects, filer-type fixed effects (company, union, worker-group), and a dummy variable for whether a petition applied as part of the NAFTA-TAA program (see text). Monetary variables are deflated to 2010 US dollars, seasonally adjusted, and winsorized at the 1% level to limit the influence of outliers. Standard errors (in parentheses) are clustered at the investigator level. ***p<0.01, **p<0.05, *p<0.10. Source: LEHD, DOL.

Table 5. Pooled Effects of TAA on Annual Earnings ($)

	OLS	**2SLS**				
	(1)	(2)	(3)	(4)	(5)	(6)
A. Post-Training						
1(TAA Approved)	-963.8***	14,474.4*	14,335.4*	14,812.3*	13,112.8**	10,255.9**
	(250.2)	(8211.1)	(8449.8)	(8383)	(6432.3)	(5086.7)
B. Pre-Training [Placebo]						
1(TAA Approved)	-150.9	405.4	313.1	169.2	266.9	333.8
	(246.7)	(480.1)	(499.8)	(514.2)	(677.8)	(660.1)
Identification Controls:						
Filer Industry FEs (4-digit SIC)	yes	yes	yes	yes	yes	yes
Investigator Concentration Controls	yes	yes	yes	yes	yes	yes
Precision Controls:						
Calendar Year & Filer Quarter FEs	yes	yes	yes	yes	yes	yes
Baseline Controls			yes	yes	yes	yes
Filer Type & NAFTA-TAA FEs				yes	yes	yes
Filer State FEs					yes	yes
Demographic Controls						yes
Number of Petitioners	∼4,300	∼4,300	∼4,300	∼4,300	∼4,300	∼4,300
Number of Workers	∼177,000	∼177,000	∼177,000	∼177,000	∼177,0000	∼177,000
Number of Observations (All Periods)	∼2,623,000	∼2,623,000	∼2,623,000	∼2,623,000	∼2,623,000	∼2,623,000

NOTES—This table reports the main effects of TAA on annual earnings, pooled in the "post" and "pre" periods separately such that each column presents two regression coefficients. Each specification corresponds to equation (2) of the paper for the "High Attachment" sample, which includes working-age individuals (22-65) making under $50,000 annually in the pre-period, with 2 years above the annual minimum wage equivalent prior to filing ($7.25 * 2,082 average working hours in 2010 ≈ $15,000). *Baseline Controls* include means over 5 to 40 quarters prior to the TAA petition decision for the following variables: worker earnings, quarters employed, worker tenure, and initial employer and state earnings. *Demographic Controls* include: race and gender fixed effects, a cubic in age fully interacted with baseline earnings and gender indicators. Filer-type fixed effects indicate whether a petition was filed by a company, union, worker-group, or career center (omitted). NAFTA-TAA is a dummy variable for whether a petition applied as part of the NAFTA-TAA program (see text). Monetary variables are deflated to 2010 US dollars, seasonally adjusted, and winsorized at the 1% level to limit the influence of outliers. Standard errors (in parentheses) are clustered at the investigator level. ***p<0.01, **p<0.05, *p<0.10. Source: LEHD, DOL

Table 6. Pooled Effects of TAA on Quarters Employed

	OLS	**2SLS**				
	(1)	(2)	(3)	(4)	(5)	(6)
A. Post-Training						
1(TAA Approved)	-0.0127	1.368*	1.209*	1.175*	0.795**	0.642**
	(0.72)	(0.72)	(0.62)	(0.61)	(0.38)	(0.32)
B. Pre-Training [Placebo]						
1(TAA Approved)	-0.00116	0.43	0.0193	0.0157	0.0466	0.0587
	(0.01)	(0.30)	(0.05)	(0.05)	(0.08)	(0.08)
Identification Controls:						
Filer Industry FEs (4-digit SIC)	yes	yes	yes	yes	yes	yes
Investigator Concentration Controls	yes	yes	yes	yes	yes	yes
Precision Controls:						
Calendar Year & Filer Quarter FEs	yes	yes	yes	yes	yes	yes
Baseline Controls			yes	yes	yes	yes
Filer Type & NAFTA-TAA FEs				yes	yes	yes
Filer State FEs					yes	yes
Demographic Controls						yes
Number of Petitioners	∼4,300	∼4,300	∼4,300	∼4,300	∼4,300	∼4,300
Number of Workers	∼177,000	∼177,000	∼177,000	∼177,000	∼177,0000	∼177,000
Number of Observations (All Periods)	∼2,623,000	∼2,623,000	∼2,623,000	∼2,623,000	∼2,623,000	∼2,623,000

NOTES—This table reports the main effects of TAA on quarters employed, pooled in the "post" and "pre" periods separately such that each column presents two regression coefficients. Each specification corresponds to equation (2) of the paper for the "High Attachment" sample, which includes working-age individuals (22-65) making under $50,000 annually in the pre-period, with 2 years above the annual minimum wage equivalent prior to filing ($7.25 * 2,082 average working hours in 2010 ≈ $15,000). *Baseline Controls* include means over 5 to 40 quarters prior to the TAA petition decision for the following variables: worker earnings, quarters employed, worker tenure, and initial employer and state earnings. *Demographic Controls* include: race and gender fixed effects, a cubic in age fully interacted with baseline earnings and gender indicators. Filer-type fixed effects indicate whether a petition was filed by a company, union, worker-group, or career center (omitted). NAFTA-TAA is a dummy variable for whether a petition applied as part of the NAFTA-TAA program (see text). Monetary variables are deflated to 2010 US dollars, seasonally adjusted, and winsorized at the 1% level to limit the influence of outliers. Standard errors (in parentheses) are clustered at the investigator level. ***p<0.01, **p<0.05, *p<0.10. Source: LEHD, DOL

Table 7. Pooled Effects of TAA on Formal Education

	OLS	**2SLS**			
	(1)	(2)	(3)	(4)	(5)
A. Post-Training					
1(TAA Approved)	-0.0183	-0.233	-0.133	-0.081	-0.15
	(0.01)	(0.22)	(0.2)	(0.18)	(0.16)
B. Pre-Training [Placebo]					
1(TAA Approved)	-0.0106	-0.0605	-0.0159	-0.00948	-0.188
	(0.01)	(0.19)	(0.18)	(0.17)	(0.18)
Identification Controls:					
Filer Industry FEs (4-digit SIC)	yes	yes	yes	yes	yes
Investigator Concentration Controls	yes	yes	yes	yes	yes
Precision Controls:					
Calendar Year & Filer Quarter FEs	yes	yes	yes	yes	yes
Baseline Controls			yes	yes	yes
Filer Type & NAFTA-TAA FEs				yes	yes
Demographic Controls & State FEs					yes
Number of Petitioners	∼4,300	∼4,300	∼4,300	∼4,300	∼4,300
Number of Workers	∼177,000	∼177,000	∼177,000	∼177,000	∼177,000
Number of Observations (All Periods)	∼2,623,000	∼2,623,000	∼2,623,000	∼2,623,000	∼2,623,000
Baseline Education Value	2	2	2	2	2

NOTES—This table reports the main effects of TAA on formal education, pooled in the "post" and "pre" periods separately such that each column presents two regression coefficients. Ordinal education values reflect combined imputed and non-imputed decennial census estimates which are merged to the Census LEHD based on a scrambled social security identifier. 1=Some High School; 2=High School Degree; 3=Some College; 4=College Degree. Each specification corresponds to equation (2) of the paper for the "High Attachment" sample, which includes working-age individuals (22-65) making under $50,000 annually in the pre-period, with 2 years above the annual minimum wage equivalent prior to filing ($7.25 * 2,082 average working hours in 2010 $\approx$ $15,000). *Baseline Controls* include means over 5 to 40 quarters prior to the TAA petition decision for the following variables: worker earnings, quarters employed, worker tenure, and initial employer and state earnings. *Demographic Controls* include: race and gender fixed effects, a cubic in age fully interacted with baseline earnings and gender indicators. Filer-type fixed effects indicate whether a petition was filed by a company, union, worker-group, or career center (omitted). NAFTA-TAA is a dummy variable for whether a petition applied as part of the NAFTA-TAA program (see text). Monetary variables are deflated to 2010 US dollars, seasonally adjusted, and winsorized at the 1% level to limit the influence of outliers. Standard errors (in parentheses) are clustered at the investigator level. ***p<0.01, **p<0.05, *p<0.10. Source: LEHD, DOL

Table 8. Event Year Coefficient Estimates by Heterogeneity Groupings

	Earnings ($)		**Pr(Stays in Initial CZ)**		**Pr(Stays in Initial NAICS)**	
Event Year (τ)	(1) High Training Duration	(2) Low Training Duration	(3) High Shock	(4) Low Shock	(5) High Shock	(6) Low Shock
0	-9,357.12**	-5,432.05	-0.3231	-0.1044	0.0052	-0.2405*
1	-4,015.65	459.38	-0.3810*	-0.4943**	-0.2287	-0.5293*
2	4,324.40	10,001.72	-0.3083*	-0.3178	-0.3461*	-0.4958
3	7,629.67	12,370.85	-0.3819*	0.0704	-0.5085**	0.1071
4	6,972.06	11,692.10*	-0.3288**	0.2292	-0.5104***	0.3335
5	6,766.97*	7,730.66*	-0.3211*	0.1521	-0.4310**	0.2651
6	9,626.92**	6,971.34*	-0.2970*	0.2007	-0.3746**	0.6994*
7	9,608.34**	6,317.66	-0.3172*	0.2482	-0.3941**	0.6446
8	5,931.00*	4,201.82	-0.2486*	0.2721	-0.2688*	0.5458
9	5,776.88*	4,278.90	-0.2023*	-0.0301	-0.2833*	0.4003
Identification Controls:						
Filer Industry FEs (4-digit SIC)	yes	yes	yes	yes	yes	yes
Investigator Concentration Controls	yes	yes	yes	yes	yes	yes
Precision Controls:						
Calendar Year & Filer Quarter FEs	yes	yes	yes	yes	yes	yes
Baseline Controls	yes	yes	yes	yes	yes	yes
Filer Type & NAFTA-TAA FEs	yes	yes	yes	yes	yes	yes
Demographic Controls & State FEs	yes	yes	yes	yes	yes	yes
No. of Workers	∼177,000		∼177,000		∼177,000	

NOTES—This table shows 2SLS coefficient estimates of the effect of TAA on the dependent variable and sub-group indicated for the main analysis sample—"High Labor Force Attachment". Columns (1) and (2) correspond to Figure 10. Columns (3) through (6) correspond to Figure 12. Number of displaced workers are rounded as per Census Bureau confidentiality requirements. Each point estimate is from a separate cross-sectional regression for the event year relative to the TAA petition decision ($\tau = 0$). Standard errors clustered at the investigator level. ***p<0.01, **p<0.05, *p<0.10. Source: LEHD; DOL

Appendices

Appendix A. Data Appendix

A.1 LEHD Sample Construction and TAA Matching

This appendix provides additional detail on how the matched LEHD sample of workers is constructed. The main sample consists of worker histories for separating workers associated with both approved and denied TAA petitions, flagged as "separated" if observed moving from positive to zero earnings in the TAA eligibility window.[62] Workers in each state are observed if they were employed between the following quarter and 2011q4:

- 1985q2: MD
- 1990q1: CO, IL, IN, KS, WA, MO
- 1991q1: OR, PA
- 1991q3: CA
- 1992q1: AZ
- 1992q4: FL
- 1993q1: MT
- 1995q3: NM
- 1996q1: ME
- 1997q1: WV
- 1998q1: NV, SC, TN
- 1998q3: DE
- 1998q4: IA
- 2000q1: OK
- 2002q2: DC
- 2002q3: AR

To identify TAA filers at the worker-level, I first have to identify both the correct TAA petitioner plant, and the timing of the separation event in the LEHD. I restrict attention to all petitions filed between 1990 and 2011. Matching is done in several steps:

Step 1. Match TAA petitioner list of establishments (DOL list of first-time TAA claims against establishments) separately on cleaned address and company name strings within each state, to the Standard Statistical Establishment List (SSEL) single- and multi-unit files for the calendar year of the petition and the year prior to the petition (to account for any potential filing lags or measurement error in filing dates).

Step 2. Keep SSEL establishments if the matched address or company name is unique in a given state-year cell. By focusing on one-to-one matches, this circumvents problems arising from matching one TAA establishment address to a Census location containing many establishments at the same address (such as an office building). This is especially important when there is a small expected effect size but large variation in the outcome variable, or compliance is imperfect, as is known to be the case with TAA (Schochet et al., 2012). The sample thus reflects a very conservative match, but also one with external validity representative of rural, stand-alone, manufacturing plants.

Step 3. Draw three random samples of 100 matches (without replacement) and manually verify rate at which SSEL establishment names match TAA establishment names.[63]

Step 4. Using the LEHD Employer Characteristics FIle (ECF), assign all units at each SEIN (State Employer Identification Number) a matched SSEL establishment (if a match exists) by merging the two datasets on federal EIN (Employer Identification Number using the LEHD-provided *es_ein* variable).

[62] This definition of separation allows for a greater sample size, however all results are robust to using "mass layoff" definitions in which greater than 30% of employees are laid off, "displacement" definitions in which workers make less than 70% of their prior earnings in the post-period, and restrictions that only keep workers who are unemployed for more than 2 quarters (eliminating potential measurement error).

[63] Match rates for all three samples are greater than 98%, with the residual owing to likely discrepancies between parent and subsidiary names which could not be reconciled between the two datasets.

Then, using the SEIN, pull the full worker history for any worker that separated in the TAA eligibility window (shown in Section 2.2) from the Employee History File (EHF). Workers can furnish returns from multiple employers, but earnings are aggregated (collapsed) such that each worker only has one earnings observation per quarter.

Importantly, a worker is marked unemployed if she is observed subsequently reemployed after having a missing earnings observation. However, I cannot distinguish between the following cases if the worker is not observed as reemployed (censored):

- Worker is discouraged / out of labor force
- Worker moved to state outside approved sample (otherwise workers can be tracked across states)
- Worker is on long-term disability / has retired / is deceased
- Worker is self-employed

A.2 Tradability and Offshorability Indices

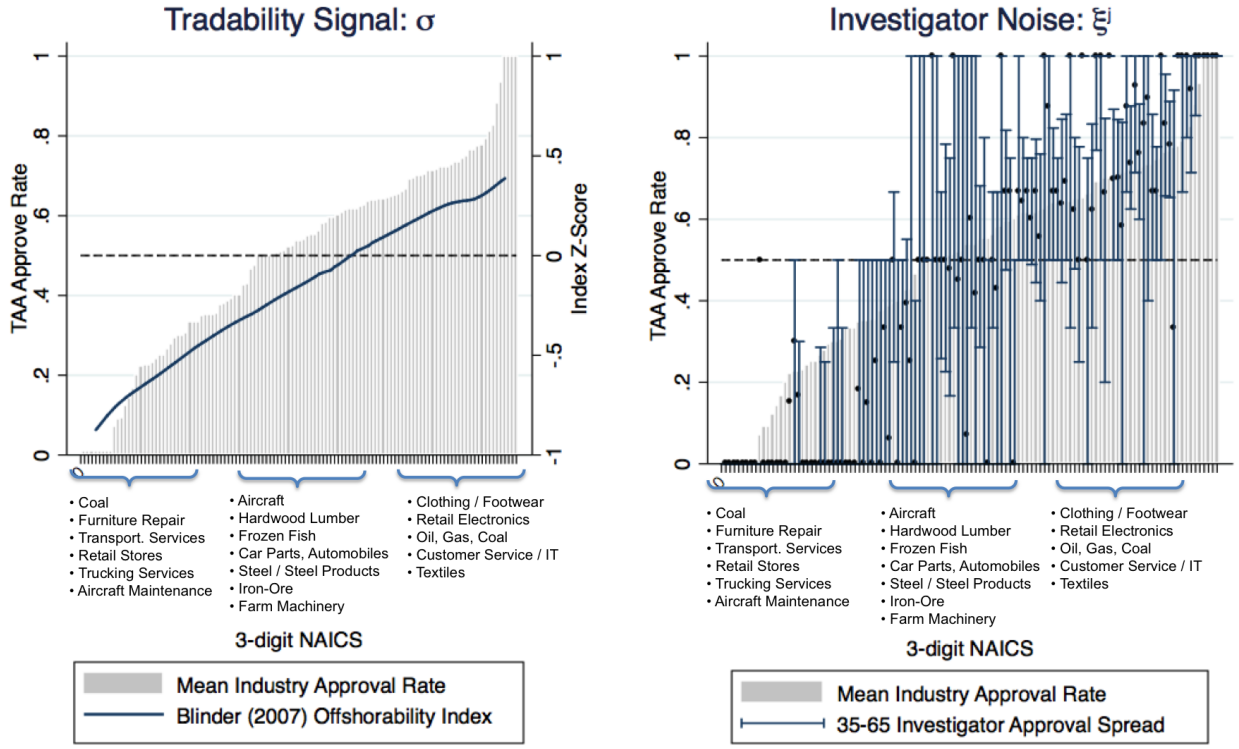
In some sense, the mean approval rate across different TAA industries forms an implicit index of industry tradability or offshorability as perceived by TAA case investigators. To show how well investigator approval rates map to known indices in the literature, I use three industry-level measures.

I first take the offshorability score assigned by Blinder (2007) to each occupation code (O*Net OCC 10.0, last revised in 1991), which also contains Department of Labor 6-digit Standard Occupational Classification (SOC) codes. Using the BLS Occupational Employment Statistics (OES) national industry estimates that show the most common SOC occupations for each 6-digit NAICS industry, I compute a weighted sum of the offshorability index within each industry, multiplying the share of employees in a given occupation times the offshorability score. I then divide this by its standard deviation to have a comparable standardized measure across industries.

I repeat this exact same procedure for 1990 occupational codes provided by Autor and Dorn (2013), who base their offshorability measures on face-to-face and on-site interaction requirements used in Firpo, Fortin, and Lemieux (2011). Finally, Mian and Sufi (2014) provide their own measure of tradability based on geographic concentration. Normalizing all three measures, I can compare them and how well they correlate with mean TAA approval rates across 3-digit NAICS codes.

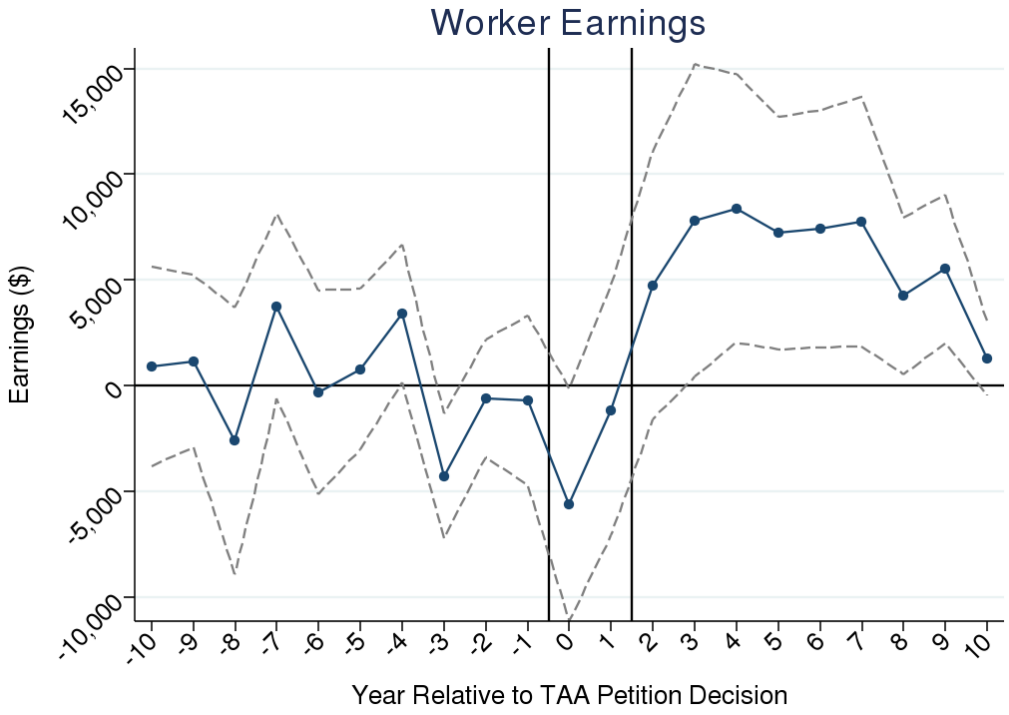
Appendix B. Additional Figures

Figure B.1 Local Average Treatment Effect (LATE) Compliers



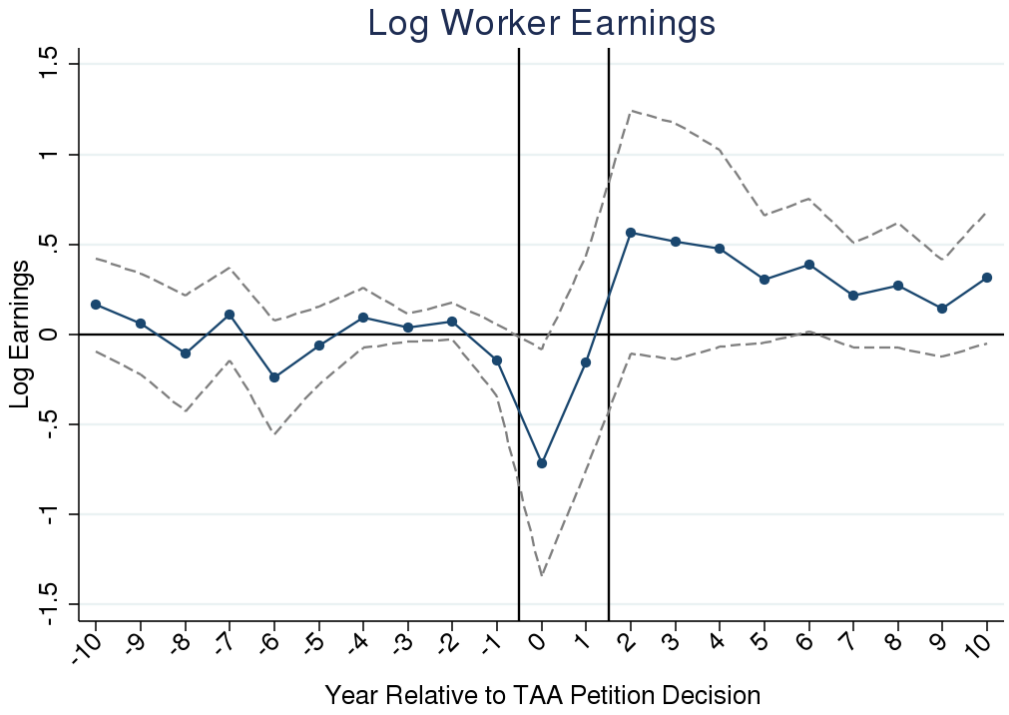
NOTES—This figure highlights the types of workers and firms that are likely to comply with the TAA investigator leniency instrument—i.e. LATE compliers—those indicated by the middle bracket in each panel. The left panel orders mean industry approval rates from lowest to highest by 3-digit NAICS codes, calculated over all TAA cases from 1974 to 2016. Overlaid is a standard measure of tradability calculated from Blinder (2007) for the same industries. (See Appendix A.2 for details on the construction of this index at the industry level, as well as other tradablility measures from Mian and Sufi (2014) and Autor and Dorn (2013).) The right panel shows within-industry investigator variation around the same mean approval rates, where the median is indicated with a black dot and whiskers reflect the 35th to 65th leniency percentiles within each industry. Each 1(TAA) approval decision can be thought of as a latent variable sum of an observable tradability signal (left) and subjective adjudication noise (right). 85% of workers are associated with leniency rates between 20% and 80% (i.e. marginal cases), rather than with industries that are generally always denied or approved benefits. Source: DOL (OTAA) petition database attained via FOIA request

Figure B.2 Dynamic Effects of TAA on Annual Earnings, Full Sample



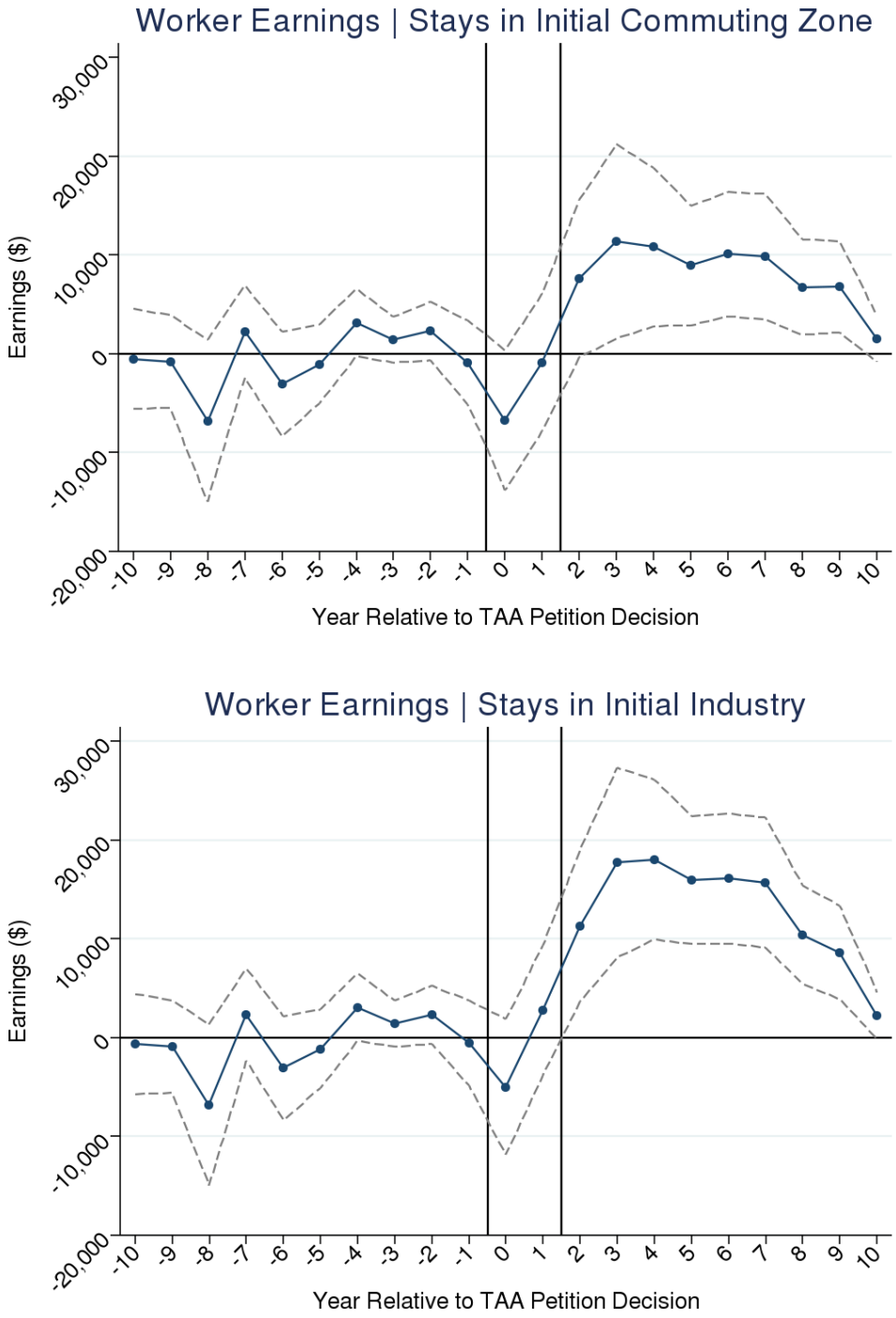
NOTES—This plots shows 2SLS coefficient estimates of the effects of TAA on annual earnings for the unrestricted "High and Low Labor Force Attachment" sample (287,000 displaced workers), as robustness to the preferred sample estimates in Figure 7. Coefficients are estimated for each event year relative to the TAA petition decision ($\tau = 0$), such that each point estimate is from a separate cross-sectional regression. Vertical lines partition the support into pre-TAA, during-TAA, and post-TAA periods. Sample restrictions and variable definitions are identical to those described in Table 5, column 6 (the paper's preferred earnings specification), except workers are not required to have 2 years above the annual minimum wage equivalent prior to filing. Dotted lines represent 90% confidence intervals with standard errors clustered at the investigator level. Source: LEHD; DOL

Figure B.3 Dynamic Effects of TAA on Log Earnings



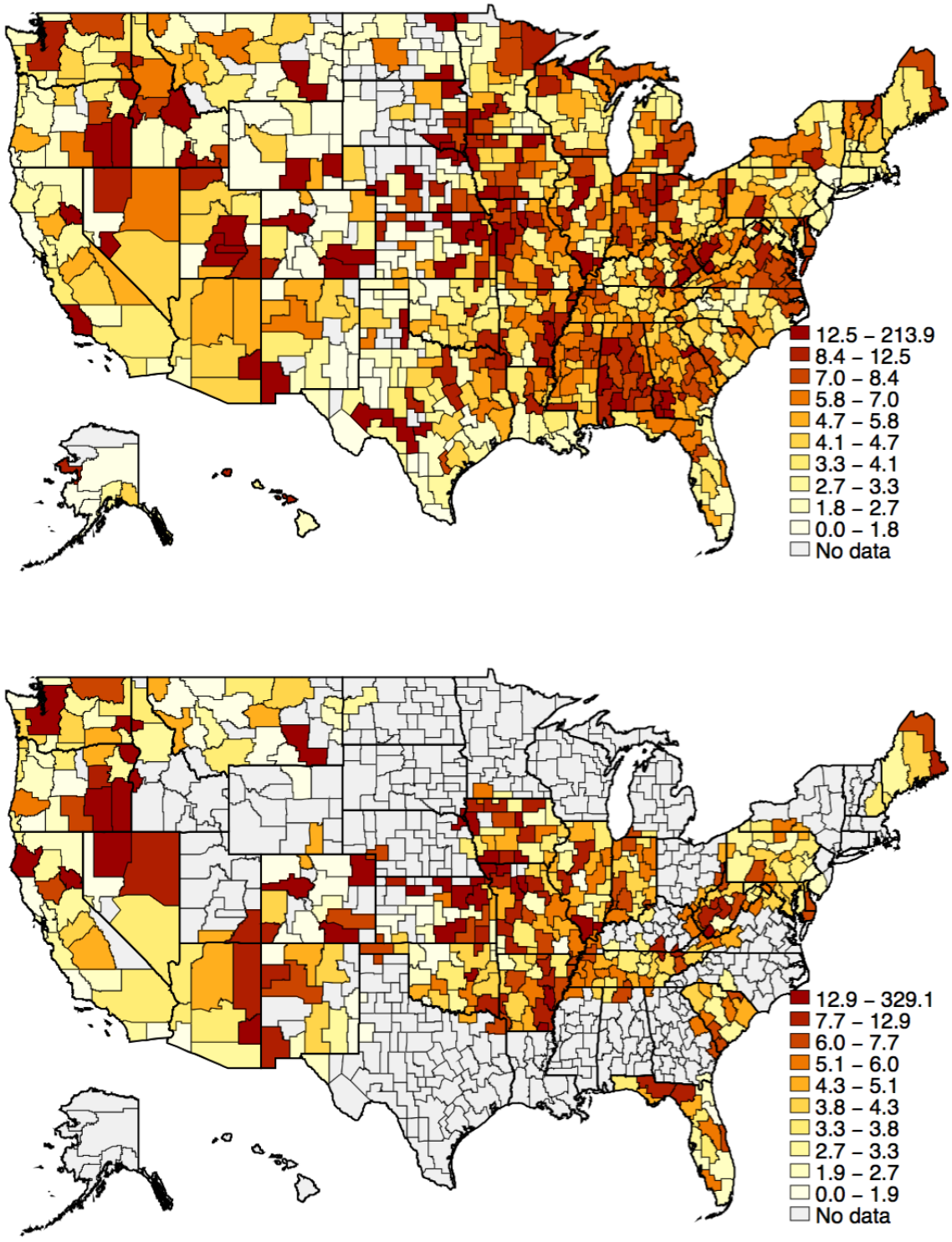
NOTES—This plot shows 2SLS coefficient estimates for the effects of TAA on log earnings as an alternative specification to (Figure 8 (effects on quarters employed). Estimates are shown dynamically across event years relative to the TAA petition decision ($\tau = 0$). Each point estimate is from a separate cross-sectional regression by event year. Vertical lines partition the support into pre-TAA, during-TAA, and post-TAA periods. Sample restrictions and variable definitions are identical to those described in Table 5, column 6, except non-zero earnings quarters are eliminated by logging. Dotted lines represent 90% confidence intervals with standard errors clustered at the investigator level. Source: LEHD; DOL

Figure B.5 Effects of TAA on Earnings for CZ and Industry "Stayers"



NOTES—This figure shows 2SLS estimates of the effects of TAA for workers who stay in their initial commuting zones (top) and 2-digit NAICS industries (bottom) in the post-period. This *ex-post* definition of mobility is used as a subgroup in a specification similar to equation (5) of the draft, where only coefficients on stayers are plotted here. Stayers comprise approximately 53% and 44% of the sample for commuting zones and industries respectively. Each point estimate is from a separate regression by event year. Vertical lines partition the pre-TAA, during-TAA, and post-TAA periods. Sample restrictions and variable definitions are identical to those described in Table 5, column 6. Dotted lines represent 90% confidence intervals with standard errors clustered at the investigator level. Source: LEHD; DOL

Figure B.6 Workers Filing for TAA: All (Top) versus LEHD Sample (Bottom), Weighted by 2010 Zip Code Population (1,000 people)



NOTES—These maps show the cumulative number of TAA filers by 1990 commuting zone (geographies taken from Dorn (2009)). The top sample reflects the universe of TAA filers from 1974 to 2016. The bottom sample displays all filers in 24 LEHD-approved states from 1990 to 2011, forming the basis of the matched analysis sample. Both maps display cumulative numbers by quintile. Source: DOL (OTAA) petition database attained via FOIA request; US Census Bureau 2000 decennial census

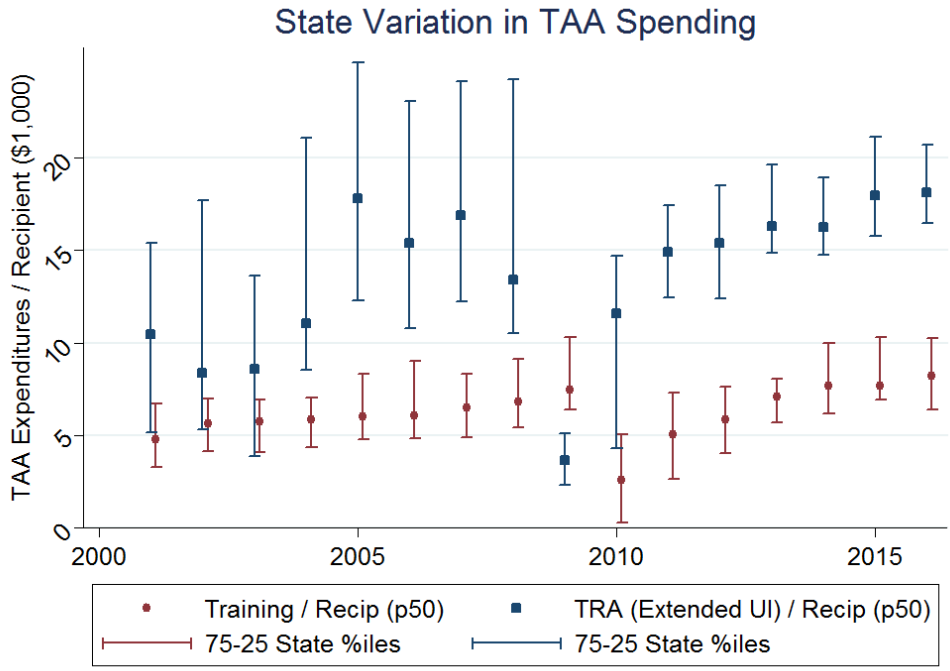
Appendix C. Additional Tables

Table C.1 Dynamic Earnings Coefficient Estimates by Event Year

	High Attachment		**High & Low Attachment**	
Event Year (τ)	β_τ	Std. Error	β_τ	Std. Error
0	-8,117.04*	(4376.39)	-5,632.83*	(3,333.45)
1	-2,658.47	(4297.99)	-1,160.72	(3590.28)
2	6,350.23	(4789.55)	4,748.69	(3832.99)
3	9,259.34	(5885.42)	7,810.82*	(4464.60)
4	8714.95*	(4781.49)	8,374.10**	(3834.50)
5	7186.63*	(3692.48)	7,212.00**	(3,318.52)
6	8374.69**	(3790.21)	7,413.26**	(3,378.17)
7	7948.48**	(3871.13)	7,757.00**	(3,552.50)
8	4845.18*	(2808.49)	4,235.00*	(2,221.53)
9	5012.91*	(2734.88)	5,511.12***	(2,093.29)
10	978.67	(1421.35)	1,294.62	(1,041.89)
Identification Controls:				
Filer Industry FEs (4-digit SIC)	yes		yes	
Investigator Concentration Controls	yes		yes	
Precision Controls:				
Calendar Year & Filer Quarter FEs	yes		yes	
Baseline Controls	yes		yes	
Filer Type & NAFTA-TAA FEs	yes		yes	
Demographic Controls & State FEs	yes		yes	
No. of Workers	∼177,000		∼287,000	
No. of Observations (Total)	∼2,623,000		∼4,192,000	

NOTES—This table shows 2SLS coefficient estimates of the effect of TAA on annual earnings by event year. "High Attachment" estimates correspond to the main analysis sample of workers that are highly attached to the labor force (see Figure 7 for corresponding graph and sample restrictions). "High and Low Attachment" estimates correspond to the unrestricted sample (see Figure B.1 for corresponding graph and sample restrictions). Number of displaced workers are rounded as per Census Bureau confidentiality requirements. Each point estimate is from a separate cross-sectional regression for the event year relative to the TAA petition decision ($\tau = 0$). Standard errors clustered at the investigator level. ***p<0.01, **p<0.05, *p<0.10. Source: LEHD; DOL

Figure B.4 Composition of TAA Spending Across States & Time



NOTES—This figure displays median TAA expenditures on training and extended UI (Trade Readjustment Allowances (TRA)), along with their inter-quartile range across states within each year. Data reflects spending on recipients known to have participated for at least one day in training or TRA. The large variation in extended UI rates depend partly on different initial UI generosities and formulae across states (see Department of Labor Unemployment Insurance Data Summary), and partly on legislative changes and automatic stabilizers implemented during the Great Recession. Source: DOL Trade Act Participant Report (TAPR), 2001 to 2016