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Abstract

We evaluate the employment effect of green investments from the American Recovery and Reinvestment Act (ARRA). Most job creation from green ARRA investments emerged in the post-ARRA period (2013-2017) and mostly benefited areas with a greater prevalence of pre-existing green skills. On average, each $1 million of green ARRA created approximately 10 long-run jobs, but the job creation effect doubled in regions in the last quartile of green skills distribution. New jobs are primarily in construction and in occupations performing green tasks. Manual workers are the main winners in terms of employability, but not of wage gains.

Keywords: employment effect, green subsides, American Recovery Act, heterogeneous effect, distributional impacts

JEL Codes: E24, E62, H54, H72, Q58

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I. Introduction

There is growing interest in green fiscal stimuli. Investing in the green economy has been identified as a strategic area of intervention both as a response to the climate crisis as well as the economic crisis induced by the Covid-19 pandemic (e.g. Helm 2020; Agrawala et al., 2020). A leading example is the European Commission's European Green Deal (EGD), first proposed in December 2019, a few months before the COVID-19 pandemic outbreak. The EGD puts a green fiscal stimulus at the center of the European Union's growth strategy to achieve social, economic, and environmental goals. Funding for the EGD will be expanded in the context of the COVID-19 plans within the Recovery Plan for Europe (NextGenerationEU, €750 billion for 2021-2014).[1] Similar proposals have been made by the International Energy Agency, the International Monetary Fund and some Democrats in the US.

Among the goals of most green fiscal stimuli is creating new green jobs for workers potentially displaced by a green transition. Adverse impacts of green initiatives on manual labor are of particular concern for policy-makers, given the secular decline in their employability and wages driven by automation and globalization (Autor et al., 2003; Autor et al., 2013). While the net effect of environmental policies on employment is typically small (Morgenstern et al., 2002; Hafstead and Williams, 2018; Metcalf and Stock, 2020), recent work finds evidence of job losses concentrated in polluting industries (Greenstone, 2002, Kahn and Mansur, 2013) and among unskilled workers (Yip, 2018; Marin and Vona, 2019).

[1] In the State of the Union speech of September 16th 2020, the President of the European Commission Ursula von der Leyen said that "37% of NextGenerationEU will be spent directly on our European Green Deal objectives". https://ec.europa.eu/commission/presscorner/detail/en/SPEECH_20_1655. Importantly, the distributional impacts for most affected workers and regions of the EGD are directly tackled by a Just Transition mechanism of €17.5 billion.

The success of green fiscal stimuli thus depends, at least in part, on whether these investments create new jobs and whether such jobs are available to workers negatively impacted by a green transition. While much work evaluates the effect of policies imposing a cost on pollution (either through standards or prices) on labor markets, almost no work considers the potential of green subsidies opening up new employment opportunities in the so-called green economy.[2] We provide the first rigorous assessment of one such push for the green economy, namely the green part of the American Recovery and Reinvestment Act (ARRA, henceforth). The full stimulus package included over $350 billion of direct government spending, and an additional $260 billion in tax reductions (Aldy, 2013). We focus on the direct spending targeted at green investments, which constituted approximately 19% of all direct government spending in ARRA (Appendix Figure A1). Examples of such spending include Department of Energy (DOE) block grants to states to support energy efficiency audits and retrofits, investments in public transport and clean vehicles, and Environmental Protection Agency (EPA) spending to clean up brownfield sites. Because a large share of green spending was devoted to public investments, green ARRA may have a cumulative effect stretching beyond the stimulus period (Council of Economic Advisers, 2013, 2014). We thus differentiate between the short- and long-term effect of green ARRA. We evaluate the employment gains triggered by the green stimulus, its heterogeneous effect depending on the level of local green capabilities and the way in which the green stimulus has affected different sectors and groups of workers.

[2] The only exception is the related paper of Vona et al. (2019), which uses similar data. Following Moretti (2010), they estimate the additional number of jobs indirectly created in the local economy by a new green job. We extend their work by estimating the direct effect of green subsidies, its time-profile and the heterogeneous effects across workers, sectors and communities. Examples of paper evaluating the costs of policy include Greenstone (2002), Walker (2011), Ferris et al. (2014), Curtis (2018) and Vona et al. (2018). For estimates of the effect of energy prices and carbon taxes, see, e.g., Kahn and Mansur (2013), Martin et al. (2014), Marin and Vona (2017, 2019), Yamazaki (2017) and Yip (2018).

Our analysis makes three contributions to the discussion of heterogeneous labor market effects. First, using data on green skills from Vona et al. (2018), we show that the effectiveness of green investments varies depending on the pre-existing skill base of a community. Theoretically, a larger pool of workers with the skills required to perform green tasks reduces mobility frictions and reallocation costs, thus improving the aggregated effect of environmental policies (Castellanos and Heutel, 2019). Second, we estimate the effects of green ARRA investments on different sectors and sets of occupations to identify those workers receiving the most benefits from green investments. Third, our focus on heterogeneous effects across different types of workers also adds to the literature on structural transformations and inequality in local labor markets (e.g., Autor et al., 2013; Acemoglu and Restrepo, 2020). A key difference between investments in the green economy, especially in building retrofitting and energy infrastructures, and in automation is that the former increase the relative demand of manual workers, while the latter decreases it. Moreover, although carbon taxation was not part of the green ARRA package, our research suggests that there may be a suitable path for reallocating manual workers displaced by carbon pricing policies in energy intensive sectors (Marin and Vona, 2019) into sectors related to the green economy, such as construction and waste management.

Our analysis also contributes to the broader literature estimating the effects of the 2009 Recovery Act. We add to the empirical literature on fiscal multipliers looking at the effect of a type of spending, i.e. in the green economy, that will become increasingly important in the future (see Chodorow-Reich, 2019 for a survey). In the spirit of recent contributions seeking to isolate the microeconomics mechanisms of the local multiplier (e.g. Moretti, 2010; Garin, 2018; Dupor and McCrory, 2018; Auerbach et al., 2019), we study the time profile of the effect, the role of key mediating factors and some mechanisms through which the green stimulus impact on the local economy.

Previous literature on other aspects of the Recovery Act exploit geographical variation in expenditures and isolate its exogenous component, and thus a causal effect, using pre-existing formulas to allocate federal funds (Wilson, 2012; Chodorow-Reich et al., 2012; Nakamura and Steinsson, 2014; Dupor and Mehkari, 2016; Chodorow-Reich, 2019). However, identifying the causal effect of the green stimulus presents three additional challenges. First, the green stimulus is small relative to the non-green stimulus. Controlling for non-green ARRA expenditures is essential, but potentially introduces another endogenous variable complicating the identification of the green ARRA effect (Angrist and Pischke, 2008). The trade-off is between an error of misspecification from not including non-green ARRA and a bias in estimating the green ARRA effect for including a bad control (non-green ARRA) correlated with the error term. We address the first challenge by including a set of twenty dummies representing each vigintile of per capita non-green ARRA. This allows us to compare the effect of green ARRA in communities that received similar levels of non-green ARRA investments and to test the robustness of our results to the exclusion of vigintiles in which the dispersion of green ARRA spending is very high or low.

Second, the allocation of green investments may depend on structural characteristics of the local economy. In general, ARRA spending targeted areas hardest hit by the recession and is endogenous by construction. The share of ARRA that is green may be further influenced by features of the economy specific to green investments, such as the presence of a federal DOE laboratory or the renewable energy potential of a region. We address these concerns through two sets of control variables capturing community characteristics prior to the Great Recession: one on general economic conditions and one on community characteristics specific to the green economy.

Third, we observe that even after controlling for these observables, areas receiving more green ARRA experienced higher employment growth before the Great Recession. We address these pre-trends in two ways. First, we allow the effect of green ARRA investments to vary across three periods: the pre-ARRA period (2005-2007); the short-term (2009-2012) and the long-term (2013-2017). We compute the long- and short-run net effect of green ARRA by subtracting its effect before 2008. Second, we use a standard shift-share instrument (e.g., Nakamura and Steinsson, 2014), where we combine the pre-sample share of different types of green spending in each community with the green ARRA shift. While neither solution is perfect, comparing the OLS and the IV results is very informative, as each approach minimizes a different source of endogeneity, which we discuss in section V.

We find that the effect of green ARRA on total employment emerges only in the long-run, with just over 10 jobs created per $1 million of green ARRA in the long-run. The effect on total employment is often imprecisely estimated in both the preferred OLS specification and the IV, but the IV amplifies pre-trends on the total effect suggesting an effect highly concentrated on compliers. The timing of green ARRA's impact differs from previous studies of other ARRA investments, which generally find larger short-term effects.

Importantly, the impact of green ARRA becomes much clearer when we explore several dimensions of heterogeneity. When looking at specific sectors or occupations we find no evidence of pre-trends, providing us with confidence that these results are more credible and easier to interpret. First, green ARRA creates more jobs in commuting zones with a greater prevalence of pre-existing green skills. Roughly speaking, $1 million of green ARRA spending creates approximately twice as many jobs in areas in top quartile of the green skills distribution than in the average commuting zone. As the presence of green skills in a community is also strongly correlated with the allocation of green ARRA subsidies, our results provide evidence of the green stimulus as a successful example of picking winners. Second, looking at specific sectors of the economy, we see the potential of a green stimulus to reshape an economy and have important distributional effects. All new jobs created are manual labor positions and are mostly in the green and construction sectors.

Even though the largest employment gains were for manual laborers with at least some college education, manual labor wages did not increase. These missing wage gains may either reflect the fact that the green stimulus was too small to offset the long-term deterioration of the bargaining power of manual workers, or the poor quality of the jobs created. While further research is required to understand the impact of green subsidies on labor market inequalities, these results suggest that the green stimulus may create new opportunities for those most affected by globalization and automation.

The remainder of the paper is organized as follows. Section II gives the necessary background on the green part of the Recovery Act. Section III presents the data used for this project as well as preliminary descriptive statistics. Section IV discusses the empirical strategy, while Section V the main results. Section VI discusses the policy implications of our study.

II. The Green component of the Recovery Act

In response to the Great Recession, the American Recovery and Reinvestment Act (ARRA) of 2009, commonly known as the stimulus package, invested over $800 billion in the forms of tax incentives and federal spending programs to stimulate the US economy. Through ARRA spending programs, federal agencies partnered with state and local governments, non-profit and private entities to help "put Americans back to work". Naturally, much of the spending programs funded projects that provide immediate job opportunities, such as highway construction, or filled state budget shortfalls to bail out the school system and save the jobs of teachers and school staff.

While the primary goal of ARRA was to stimulate macroeconomic growth and provide job opportunities, part of the funds were invested in "… environmental protection, and infrastructure that will provide long-term economic benefits" (American Recovery and Reinvestment Act of 2009). These include both direct spending intended for immediate job creation, such as Department of Energy spending for renewable energy and energy efficiency retrofits and Environmental Protection Agency grants for brownfield redevelopment, as well as tax breaks and loan guarantees for renewable energy. Our work focuses on the impact of direct spending intended for job creation, asking both whether these green investments stimulated employment and what types of workers may benefit from a green stimulus.

Among the key principles motivating infrastructure investments in ARRA was that facilitating the transition to energy efficient and clean energy economy would lay the foundation for long-term economic growth (Office of the Vice President, 2010). As a result, ARRA included more than $90 billion for clean energy activities, including $32.7 billion in Department of Energy contracts and grants to support projects such as energy efficiency retrofits, the development of renewable energy resources, public transport and clean vehicles, and modernizing the electric grid (Aldy, 2013). To meet the Obama administration's target of doubling renewable energy generation by 2012, DOE provided assistance for a large number of projects related to renewable energy; for example, the Massachusetts Clean Energy Center received $24.8 million to design, construct and operate a wind turbine blade testing facility (Department of Energy, 2010). Moreover, $3.4 billion in cost-shared grants supported the deployment of smart grid technology, generating more than $4.5 billion of co-investment (Aldy 2013). ARRA funding also supported the expansion of the

Weatherization Assistance Program, which supports low-income families for energy efficiency improvements (Fowlie et al., 2018).

The Environmental Protection Agency (EPA) oversaw most ARRA programs designated for environmental protection. The largest of these programs was $6.4 billion for Clean and Drinking Water State Revolving Funds, which are among the programs analyzed in Dupor and McCrory (2018). An additional $600 million was set aside for EPA's Superfund program to clean up contaminated sites such as the New Bedford Harbor site in Massachusetts and the Omaha Lead Site in Nebraska, to which the EPA allocated $30 million and $25 million, respectively[3] (Office of the Vice President, 2010). Another $200 million was invested in the Leaking Underground Storage Tank Trust Fund for the prevention and cleanups of leakage from underground storage tanks. Other EPA funds were allocated to improvements of infrastructures such as wastewater treatment facilities and diesel emissions reduction (Environmental Protection Agency, 2009). Differently from other ARRA programs, which were allocated according to statutory formulas based on exogenous factors such as the number of highway lane-miles in a state or the youth share of its population (e.g., Wilson, 2012), much green ARRA funding does not follow the same rules.

A. Data on ARRA awards

Our analysis covers the universe of contracts, grants and loans awarded under the ARRA between 2009 and 2012. Recipients of ARRA funding are required to submit reports through FederalReporting.gov, which include information on the amount of expenses and the description

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3 Information on active and archived Superfund sites is available at https://cumulis.epa.gov/supercpad/cursites/srchsites.cfm, last accessed May 27, 2020.

of projects.[4] We retrieved data from FedSpending.org on these records derived from reports submitted by non-federal entities who received ARRA funding.

In line with most recent evaluations of ARRA (Dupor and Mehkari, 2016; Dupor and McCrory, 2018), our unit of analysis is the local labor market, i.e. the so-called commuting zone (CZ). We aggregate county-level data into 709 Commuting Zones based on the official CZ definitions from the 2000 Decennial Census. As in Dupor and Mehkari (2016), we exclude 122 commuting zones with less than 25,000 inhabitants in 2008, which represent less than 0.5% of the US population and employment. We also drop the commuting zone pertaining to New Orleans, LA, as their employment and population data are heavily influenced by the recovery from Hurricane Katrina. Our primary estimation sample is thus constituted by 587 CZs. As the entities known as prime recipients who directly received funding from the federal government may make sub-contracts to other entities, we use the reported place of performance of prime and sub-prime recipients to allocate the dollar amount of awards to commuting zones based on the zip code.[5]

Nearly all DOE and EPA projects relate to the green economy.[6] Thus, our measure of green ARRA includes all ARRA projects from the DOE and EPA and their subordinate agencies, such as various national laboratories. All other ARRA spending is coded as non-green ARRA.[7] Table

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[4] This website is no longer use, but archived data are available at https://data.nber.org/data/ARRA/, last accessed March 6, 2020.

[5] Unlike other evaluations of ARRA, we do not consider the location of vendors when allocating funds. Our goal is to ascertain the effectiveness of green ARRA given the "greenness" of the local economy. If a recipient must use vendors from outside the local commuting zone to satisfy a need of the project due to a lack of qualified suppliers in the commuting zone, the funding has been less effective for stimulating local employment.

[6] To verify this, we checked projects with the term "oil", "gas", or "coal" in the description. None of these projects related to discovery of new sources. More commonly, they referenced reducing consumption, clean coal, carbon sequestration, or biofuels as a substitute.

[7] In addition to the EPA and DOE, a few other agencies funded investments that were plausibly green. The Department of Labor (DOL) supported four small job training programs (totaling just $496 million) that focused on energy efficiency and the renewable energy industry. Including these investments as green ARRA does not change our results. While the Department of Housing and Urban Development (HUD) also supported green building retrofits, we did not include these programs in our analysis. These do not fall under a single green program, and thus must be identified

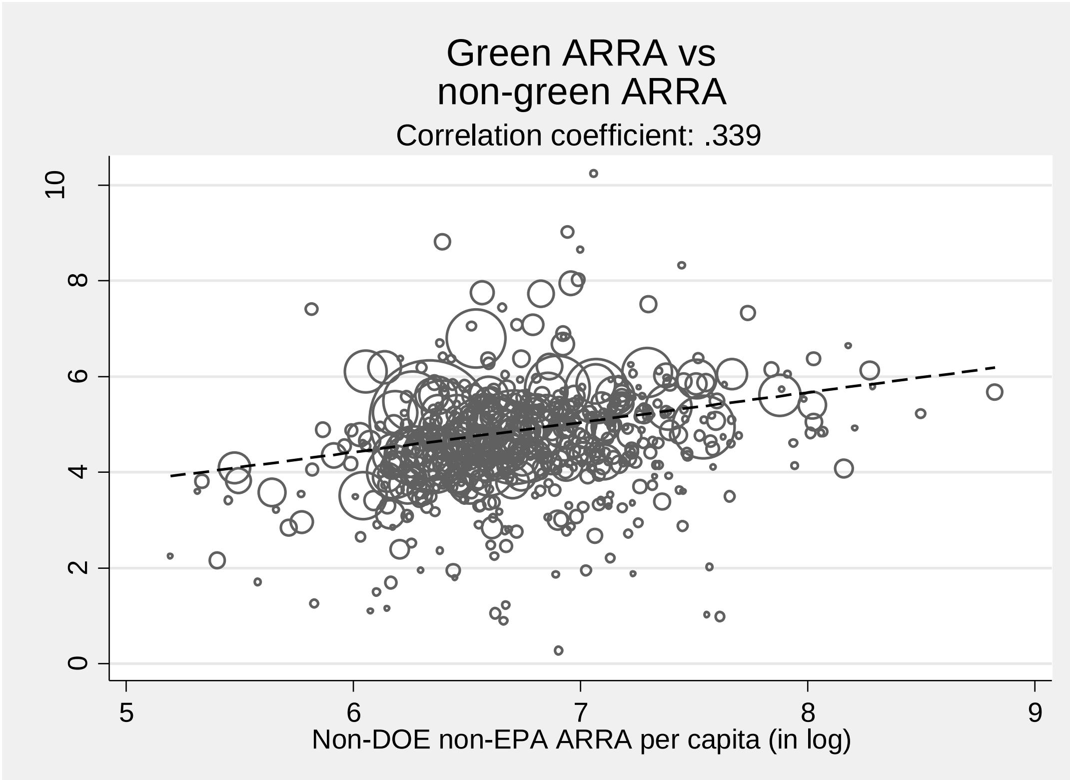
A1 in Appendix A provides descriptive data on both green and non-green ARRA. Overall, the stimulus included over $61 billion on green investments and almost $262 billion on non-green investments. Of these green investments, $52 billion come from the DOE, while just $9 billion come from EPA. Roughly 10% of green ARRA spending supported R&D. A small $228 million supported job training for green occupations.

The mean value of green ARRA and non-green ARRA per commuting zone in our sample are $103 million and $440 million dollars, respectively. The per-capita level of green ARRA and non-green ARRA are $260 and $985, respectively, based on population in 2008. We highlight the skewed distribution of both green and non-green ARRA, as the median commuting zone received only $105 and $819 dollars per capita of green and non-green ARRA awards.

Figures A2, A3 and A4 in Appendix A1 illustrate the geographic distribution of green ARRA and non-green ARRA. We do not observe any apparent, systematic patterns across geographic areas, as both areas receiving high per capita amounts (Figures A2 & A3) and areas receiving large shares of green stimulus (Figure A4) are spread throughout the country (see Table A2 for a list of commuting zones that received the largest ARRA). Figure 2 shows the correlation between green (y-axis) and non-green (x-axis) ARRA expenditure per capita for commuting zones with at least 25000 inhabitants. The bivariate correlation between the two components of ARRA is positive and somewhat strong (0.339). As such, controlling for non-green stimulus spending in a flexible way is important to accurately estimate the impact of green stimulus spending. We discuss our technique for doing so in section IV.

manually. In our attempt to label HUD investments as "green", we found that many of the "green" HUD grants were trivial – e.g. installing LED lightbulbs in a building – and should have little to no impact on green employment.

Figure 2 – Correlation between green and non-green ARRA per capita



Notes: per capita analysis based on the population of each commuting zone prior to the recession, in 2008. Linear fit and correlation coefficient weighted by CZ population in 2008. Sample: CZ with at least 25000 inhabitants.

III. Data and Descriptive Statistics

A. Data on local labor markets

We combine the ARRA data with data on local labor market conditions. These data include several control variables designed to serve two purposes. Some controls describe each commuting zone's potential exposure and resilience to the Great Recession. Others capture the stringency of environmental policies in the local labor market as well as the relative importance of green versus non-green employment in the local economy. Here we briefly describe our data on employment and green skills. Our additional outcome and control variables in the empirical analysis are collected from standard sources and are described in Appendices A2 and A3.

Data on total employment and employment by industry were retrieved from the Quarterly Census of Employment and Wages by the Bureau of Labor Statistics (QCEW-BLS). These data report average annual employment by US county and by industry. Data on the occupational composition of employment by CZ are collected from the 1% sample of the US population of the annual American Community Survey (ACS), available at IPUMS (Integrated Public Use Microdata Series, Ruggles et al., 2020). Occupation-level data for working-age population (16-64 years old) are used to build our indicators of occupational composition of the workforce.

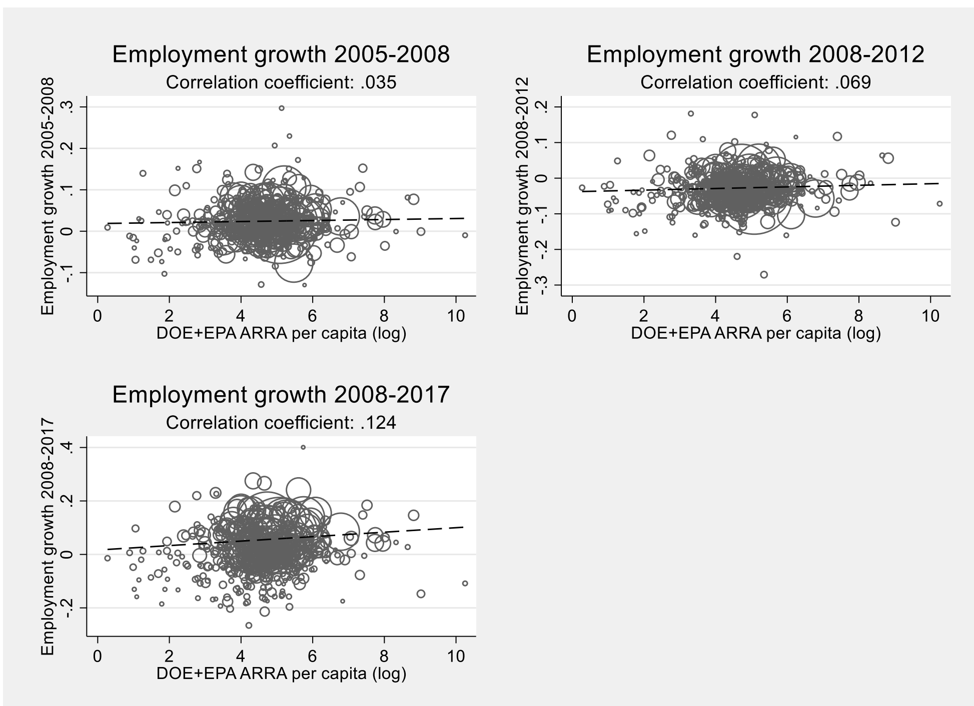
Our measures of green employment and green skills are based on Vona et al. (2018) and inspired by the task approach of labor markets (Acemoglu and Autor, 2011). For each occupation, the O*NET database provides the tasks expected of workers and the skills needed to complete these tasks. Tasks are further divided into 'general' tasks, which are common to all occupations, and 'specific' tasks that are unique to individual occupations. The *greenness* of each occupation is the share of specific tasks that are green (see also Dierdorff et al., 2009, and Vona et al., 2019). Computing the average of occupational greenness (weighted by sampling weights and annual hours worked) for each commuting zone provides the number of full time equivalent green workers in each commuting zone.

Using O*NET data on the importance of general skills to each occupation, Vona et al. (2018) identify a set of *green general skills* (GGS, hereafter "green skills") that are potentially used in all occupations, but are particularly important for occupations with high greenness. They aggregate this set of selected green skills into 4 macro-groups: Engineering and Technical, Operation Management, Monitoring, and Science. To assess the existing base of green skills, for each occupation we first compute a unique indicator of GGS as the simple average of these four macro groups. Then, using the distribution (weighted by hours worked) of green skills across different (448) occupations in 2000 (IPUMS 5% sample of the Decennial Census), we identify the occupations with green skills importance in the 75th percentile or higher across all US workers. This includes 113 occupations, which are listed in Table A3 in Appendix A2. Consistent with the types of skills included in Green General Skills, these occupations include many scientific and engineering occupations. However, not all jobs using Green General Skills are "green jobs." Green General Skills are also important in occupations such as physicians, mining machine operators, and some transportation workers. The key point is that workers in these jobs have the skills necessary to do the work required of green occupations. We compute the local green skills base in each commuting zone using microdata from the annual American Community Survey (ACS, years 2005-2017, 1% sample of the US population) from IPUMS. For each commuting zone and year, we calculate the share of total employees (weighted by sampling weights and annual hours worked) in jobs at the top quartile of green skills importance.

B. Descriptive evidence

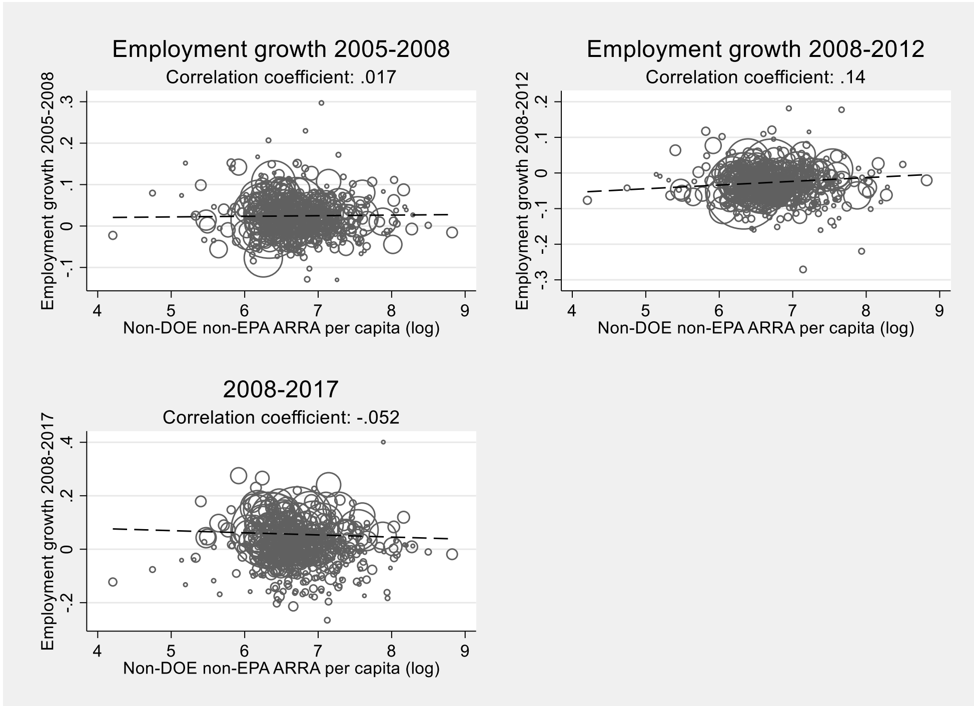
To motivate our empirical analysis, here we provide evidence on the relationship between ARRA spending and per-capita employment growth, rescaled by the population of the CZ in 2008. Figures 3 and 4 explore simple unconditional correlations between, respectively, green and non-green ARRA (2009-2012) per capita and employment growth rate for three different time windows: 2005-2008 (pre-ARRA), 2008-2012 (short term), and 2008-2017 (long term). We observe a positive but very weak correlation between ARRA spending per capita (both green and non-green) and pre-ARRA employment growth across different commuting zones. In the short-run, the unconditional correlation between non-green ARRA spending and employment growth increases substantially (0.14), while it remains very low for green ARRA spending (0.069). In the longer run the opposite is found. Green ARRA has a much stronger positive correlation (0.124) with long run employment growth, while non-green ARRA has a weakly negative correlation (-0.052). Overall, green ARRA may have been less effective at rapid job creation, which was one of the main goals of the ARRA stimulus spending. In contrast, green ARRA seems more effective in strengthening local labor markets in the long-run. This is consistent with the fact that green spending hits longer term targets such as the reshaping of the energy and transport sectors. We will explore this dynamic aspect of green ARRA effects further in our regression analysis.

Figure 3– Green ARRA per capita local spending and employment growth



Notes: change in log employment per capita (population of 2008) on log per capita green ARRA. Linear fits and correlation coefficients weighted by CZ population in 2008. Sample: CZ with at least 25000 inhabitants.

Figure 4– Non-green ARRA per capita local spending and employment/income growth



Notes: change in log employment per capita (population of 2008) on log per capita non-green ARRA. Linear fits and correlation coefficients weighted by CZ population in 2008. Sample: CZ with at least 25000 inhabitants.

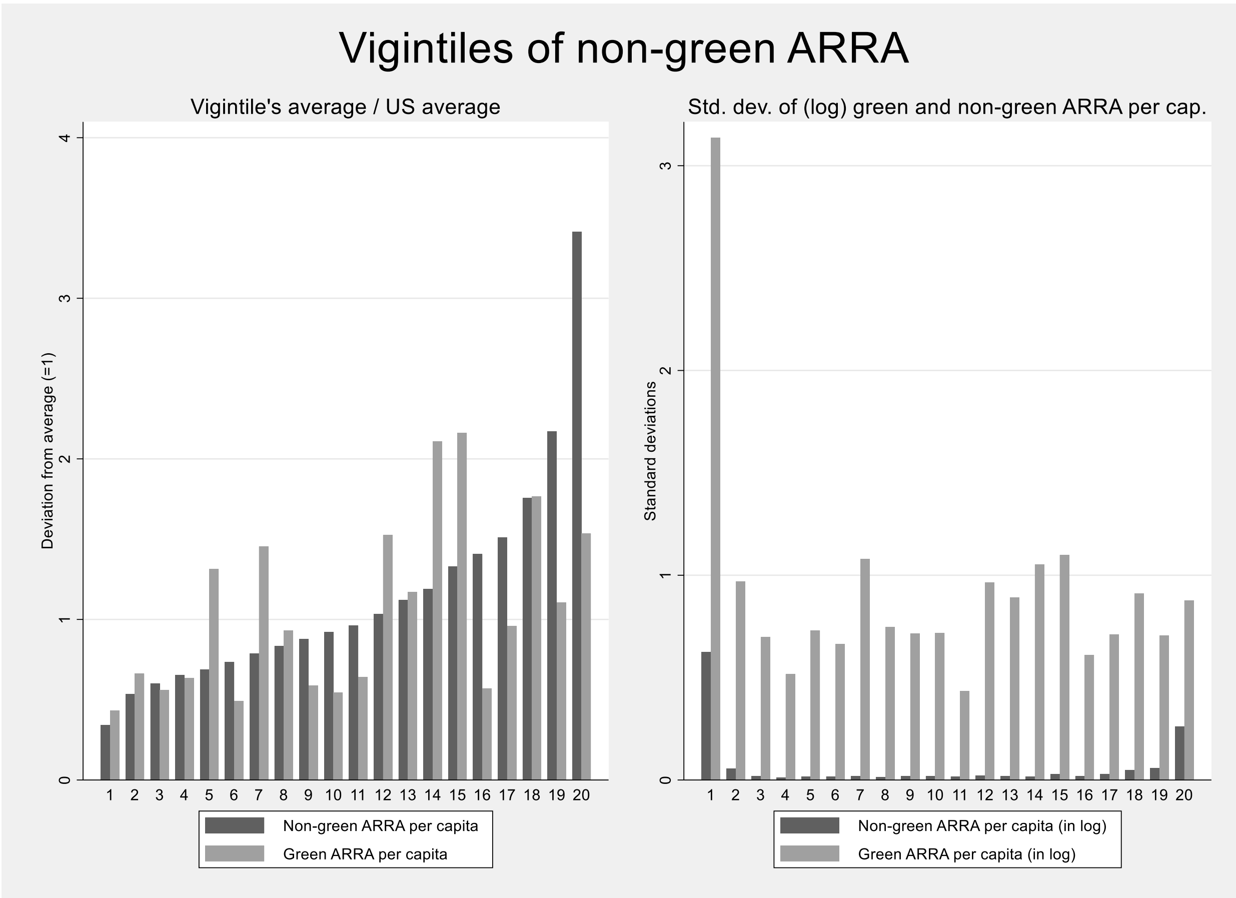
IV. Empirical Strategy

This section is organized as follows. Subsection A introduces the main endogeneity issues to estimate the effect of green ARRA on employment. Subsection B discusses our approach to tackle them.

A. Illustrating endogeneity issues

ARRA spending has been primarily designed to mitigate the effects of the Great Recession on local labor markets. Thus, it targets areas hardest hit by the recession and is endogenous by construction. For green ARRA, identification is complicated by the presence of an additional source of endogeneity. Given the significant share of green ARRA spending devoted to long-term investments and research, the allocation of such spending may have followed criteria related to other structural features of the local economy such as the presence of a federal R&D laboratory or high-tech manufacturing.

Figure 5 – Green ARRA per capita (average and SD) by vigintile of non-green ARRA per capita



Notes: unweighted vigintiles of non-green ARRA per capita across all CZ. Within-vigintiles average and SD is weighted by CZ population in 2008.

To illustrate the difference in the allocation of green and non-green ARRA as well as the source of data variation used for identification, we examine the distribution of the two types of spending along the non-green ARRA distribution. Figure 5 reports the deviations from the mean and the standard deviation of green and non-green ARRA spending per capita relative to the national mean for each vigintile of non-green ARRA spending per capita. Since non-green ARRA has been directed to areas hardest hit by the recession, the Figure illustrates the extent to which green ARRA has been allocated following a different criterion. The left panel of Figure 5 shows that the positive correlation between green and non-green ARRA masks substantial variation across vigintiles as we observe CZs with low non-green ARRA and high green ARRA or vice versa. In addition, the right panel suggests that the standard deviation of green ARRA within each vigintile is very similar across vigintiles with the exception of the first and last vigintile of non-green ARRA spending. In our econometric analysis, we will use twenty dummies for non-green ARRA vigintile to make sure that the effect of green ARRA is not capturing that of other ARRA programs. This particular functional form to treat non-green ARRA allows testing the robustness of our results to the exclusion of vigintiles in which the dispersion of green ARRA spending is very high or low or the correlation with non-green ARRA very high.

Next, we directly explore the observable characteristics of a CZ that are associated with green ARRA spending. Strong unbalances in the observable characteristics of CZs receiving different amount of green ARRA are a red spy of an unbalanced distribution also in unobservables (Altonji et al., 2005). We consider the association between the log of green ARRA spending per capita and two sets of covariates that will be used also as controls in our econometric model presented in the next section. The first set captures the economic conditions in commuting zone i before the Great Recession and are quite standard in the literature evaluating the Recovery Act (e.g. Wilson, 2012; Chodorow-Reich et al., 2012; Chodorow-Reich, 2019).[8] The second set of variables are more specific to the green economy such as the stringency of environmental

[8] We consider both the level and the pre-trends (2005-2007) in several variables such as total employment, unemployment and employment in different sectors. As in Wilson (2012), we include the pre-sample level (average 2006-2008) and long pre-trends (2000-2007) for the following variables: total employment, employment in health, public sector and education, employment in manufacturing, construction and extraction, unemployment. We also add other confounders of local labor market conditions such as pre-sample income per capita, a dummy equal one for CZ with positive shale gas production and import penetration. See data Appendix A2 for details on data sources and construction of these variables.

regulation in the local area (Greenstone, 2002), wind and solar energy potential (Aldy, 2013) and the index of the green capabilities of the workforce described in section III.A (Vona et al., 2018).[9] We also consider two alternatives to model regional fixed effects: state dummies and census division dummies as in previous literature (e.g., Dupor and Mehkari, 2016). The choice of the way of modeling time-varying regional effects is non-trivial. State fixed effects better account for unobserved shocks that are geographically concentrated and increase the precision of the estimates. But, as we will show, census division dummies mitigate pre-trends in total employment.

Table 1 shows that the inclusion of the vigintiles of non-green ARRA is not enough to eliminate differences in observable characteristics that are significantly correlated with the intensity of green ARRA spending per capita. The Table also highlights the different potential sources of endogeneity in the allocation of green ARRA: CZs receiving more green subsidies are both stronger in terms of technological expertise (workforce skills for the green economy, higher share of manufacturing employment and the presence of a federal R&D lab) and somewhat more vulnerable to the Great Recession (i.e., higher share of employment in construction, that was particularly badly hit by the Great Recession). Areas receiving more green ARRA also have a larger share of employment in the public sector. Thus, in Section V we confirm that our results are not driven by public sector employment.

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[9] As in Greenstone (2002), we use changes in the attainment status to National Ambient Air Quality Standards (NAAQS) for the six criteria air pollutants defined by the US Clean Air Act (CAA). We classify as nonattainment commuting zones in which at least 1/3 of the population resides in nonattainment counties. We also add a dummy variable to identify areas with nonattainment status for at least one of the NAAQS in 2006 and that therefore were already exposed to stringent CAA regulation. Since wind and solar energy received other types of support from the federal and state governments, including tax credits and loan guarantees as part of ARRA (Aldy, 2013), we add proxies for the wind and solar potential interacted by year fixed effects. We include a dummy equal one for areas hosting a public R&D lab and the log of local population as Vona et al. (2019) shows that is highly correlated with the size of the green economy in metropolitan areas. Finally, to proxy for the green capabilities of each CZ, we add the share of workers using intensively green general skills, i.e. skills most relevant in green jobs (see Vona et al., 2018 for details on the green skill measures). This is computed as the share of workers in the local workforce above the 75th percentile of the national distribution of green skills in 2006. See data Appendix A2 for details on data sources and construction of these variables.

Table 1 – Drivers of green ARRA

Dep var: Green (EPA+DoE) ARRA per capita (in log)	(1)	(2)	(3)	(4)
Share of empl with GGS>p75 (year 2005)	5.0404**	5.8792***	5.0980**	5.0162**
	(2.4513)	(2.0838)	(2.3752)	(2.0208)
Population 2008 (log)	0.0784	0.0096	0.0556	0.0754
	(0.1127)	(0.1027)	(0.0808)	(0.0815)
Income per capita (2005)	-0.0107	-0.0018	-0.0248*	-0.0193
	(0.0195)	(0.0140)	(0.0142)	(0.0122)
Import penetration (year 2005)	-9.8562	-19.9630*	-2.4478	-9.7260
	(11.4773)	(11.2314)	(12.7723)	(11.2876)
Pre trend (2000-2007) employment tot / pop	1.1954	-1.1082	0.6946	0.9862
	(6.2745)	(6.0718)	(4.3026)	(4.0509)
Pre trend (2000-2007) empl manufacturing / pop	-6.2834	-10.0143	-7.8693	-8.8684
	(9.0383)	(9.4050)	(7.1939)	(6.8436)
Pre trend (2000-2007) empl constr / pop	-3.6818	2.9795	-12.5936	-9.3829
	(20.0142)	(17.9305)	(13.8891)	(13.4116)
Pre trend (2000-2007) empl extractive / pop	-6.7312	12.2994	-3.2715	7.4862
	(13.4376)	(18.3117)	(13.2675)	(16.8649)
Pre trend (2000-2007) empl public sect / pop	3.0786	-0.3082	1.0662	-1.4996
	(11.8303)	(10.5796)	(10.2532)	(8.7942)
Pre trend (2000-2007) unempl / pop	-2.1602	-28.7105	11.5426	1.4942
	(24.1273)	(26.5734)	(15.5848)	(15.2373)
Pre trend (2000-2007) empl edu health / pop	4.4751	2.3869	6.3627	3.7671
	(6.7101)	(6.1369)	(5.0584)	(5.0259)
Empl manuf 2008 / pop	8.7023**	9.4260***	5.1873	6.9002**
	(4.0926)	(3.4736)	(3.5585)	(2.8822)
Empl constr 2008 / pop	41.1716***	37.2219***	47.6291***	50.6920***
	(14.2794)	(10.4966)	(13.0516)	(11.1181)
Empl extractive 2008 / pop	4.9761	-7.0123	6.2739	-2.6931
	(9.4237)	(8.0469)	(10.6118)	(8.2643)
Empl public sect 2008 / pop	22.2902**	19.9794**	14.1292*	8.6496
	(8.8124)	(8.7676)	(7.5084)	(7.0802)
Unempl 2008 / pop	14.4107	13.2134	22.7398	23.9237
	(28.5689)	(23.8820)	(21.9104)	(16.7226)
Empl edu health 2008 / pop	0.3800	0.6012	1.7704	0.1246
	(4.0785)	(2.9813)	(3.6191)	(2.4245)
Shale gas extraction in CZ	0.0269	0.2149	0.1399	0.2981**
	(0.1876)	(0.1541)	(0.1451)	(0.1206)
Potential for wind energy	-0.1145	-0.0844	-0.0495	-0.0688
	(0.1641)	(0.1659)	(0.1164)	(0.1311)
Potential for photovoltaic energy	-0.0086	0.0728	0.0475	0.1672**
	(0.1806)	(0.1299)	(0.1006)	(0.0759)
Federal R&D lab	0.4537	0.4573*	0.4632**	0.3713*
	(0.2855)	(0.2312)	(0.2113)	(0.1851)
CZ hosts the state capital	0.1267	-0.2863	0.2873	-0.0938
	(0.2287)	(0.2349)	(0.1802)	(0.1762)
Nonattainment CAA old standards	-0.2144	-0.1511	-0.0976	-0.1605
	(0.1904)	(0.1619)	(0.1702)	(0.1654)
Nonattainment CAA new standards	0.1927	0.2604*	0.0997	0.0963
	(0.1907)	(0.1497)	(0.1373)	(0.1162)
State fixed effects	Yes	Yes	No	No
US Census Division fixed effecs	No	No	Yes	Yes
Vigintiles of non-green ARRA per capita	No	Yes	No	Yes
R squared	0.3367	0.4314	0.2803	0.3782
N	587	587	587	587

The last diagnostic concerns the presence of pre-trends in our data: the possibility that employment growth before the Great Recession differs depending on the level of green ARRA received, even after controlling for observable commuting zone characteristics. We check for pre-trends using an event study framework. Including observations from 2005-2007 allows us to test whether areas receiving more per capita green ARRA spending experienced higher employment growth prior to the Great Recession, conditional on our set of controls including the vingintiles of non-green ARRA. As we show in Section V, we observe pre-trends for total employment, but only when including state fixed effects. That green ARRA may have gone disproportionately to areas growing faster before the Great Recession is not surprising given that the characteristics that define areas receiving more green ARRA are usually associated with sustained employment growth, such as the presence of an R&D lab or of manufacturing activities. Importantly, we do not observe pre-trends for the types of employment most affected by green ARRA: green employment and manual labor employment, making us confident that results for these variables are more credible and easier to interpret than results for total employment.

In sum, while the role of unbalances in the covariates can be mitigated by directly testing the robustness of the results to the exclusion of areas with R&D labs, the presence of pre-trends in some cases requires greater care to provide an accurate estimate of the effect of green ARRA on employment. We discuss the possible solution to this problem in the next section.

B. Estimating equation and instrumental variable strategy

Our main econometric model is an event-study model that jointly estimates the effects of green ARRA for years before and after the crisis. The first main advantage of this approach is that we can explicitly tackle the potential pre-trends discussed above. The second advantage is being able to assess whether the effect of green ARRA lasts beyond the stimulus period, possibly generating a virtuous circle of green investments. Our dependent variable is the long-difference between our measures of per-capita employment in year *t* relative to our base year of 2008.[10] So that the value can always be interpreted as growth in employment, we define the dependent variable as follows:

$$\Delta \ln(y_{i,t}) = ln\left(\frac{y_{i,2008}}{pop_{i,2008}}\right) - ln\left(\frac{y_{i,t}}{pop_{i,2008}}\right) = ln\left(\frac{y_{i,2008}}{y_{i,t}}\right) \qquad \text{if t < 2008}$$

$$\Delta \ln(y_{i,t}) = ln\left(\frac{y_{i,t}}{pop_{i,2008}}\right) - ln\left(\frac{y_{i,2008}}{pop_{i,2008}}\right) = ln\left(\frac{y_{i,t}}{y_{i,2008}}\right) \qquad \text{if t >2008}$$

Using this, we estimate the following equation for the 587 commuting zones in our primary estimation sample:

$$\Delta ln(y_{it}) = \alpha + \sum_t \beta_t ln\left(\frac{GreenARRA_i}{pop_{i,2008}}\right) + \sum_t \mathbf{X}'_{it_0}\boldsymbol{\varphi}_t + \sum_t \mathbf{G}'_{it_0}\boldsymbol{\vartheta}_t + \mu_{i\in v,t} + \eta_{i\in c,t} + \epsilon_{it}, \qquad (1)$$

where $\epsilon_{i,t}$ is an error term, $\mathbf{G}'_{it_0}$ are controls specific to the green economy $\mathbf{X}'_{it_0}$ are controls used in previous ARRA evaluations (see footnotes 11 and 12 for details); $\mu_{i\in v,t}$ are period-specific dummies for the vigintiles of non-green ARRA spending and $\eta_{i\in c,t}$ are period-specific region fixed effects, i.e. census division fixed effects or state fixed effects.

We estimate equation (1) by stacking all years together, but we allow the coefficient of green ARRA and of all the other covariates, including region fixed effects and the vigintiles for non-green ARRA, to vary only among three periods: pre-ARRA (2005-2007); the short-term (2009-2012) and the long-term (2013-2017). This reduces the number of coefficients to be estimated, which is important to assess the role of mediating factors of green ARRA effects, such

[10] Employment is either green employment, total employment or employment in a particular sector (construction, manufacturing, etc.) or occupation (managers, manual workers, etc.). See Appendix A3 for more details on data sources and measurement of our dependent variables.

as availability of the right green skills in the local labor market. To visually convey our main result, we also plot the green ARRA coefficients estimated on a yearly frequency through equation (1).

The main variable of interest is green ARRA spending, also rescaled by total population in 2008. While effective green spending spanned several years between 2009 and 2012, nearly all outlays were announced in 2009 (see, e.g. Figure 2 in Wilson, 2012). Therefore, we build a time invariant measure of green spending as the total spending across those four years.

We take a log transformation for both our dependent and main explanatory variable to account for the skewness in their respective distributions. In all regressions, we cluster standard errors at the state-level, using the state of the main county in each commuting zone. We cluster at the state level because the boundaries of local labor market can be larger than the commuting zone perimeter, especially in post-recession times where workers are forced to search for a job in a larger area. This results in slightly more conservative standard errors than if we cluster at the commuting zone level. We weight observations using population level in 2008.

Given the unbalances in the covariates shown in Table 1 and the possible presence of pre-trends discussed earlier, we cannot assume that the allocation of green ARRA spending to commuting zones is quasi-random, even after including our rich set of controls. The pre-trend effect $\hat{\beta}_{pre}$ reflects the presence of unobserved variables that are correlated with both the allocation of green ARRA and the outcome variables. Thus, we compute the long- and short-term effect of green ARRA by subtracting its effect before 2008. That is: $\hat{\beta}_{short} - \hat{\beta}_{pre}$ and $\hat{\beta}_{long} - \hat{\beta}_{pre}$ can be interpreted as the net effect of green ARRA in the short- or long-run, respectively.

The credibility of such differences to estimate the effect of green ARRA rests upon an untestable assumption regarding the functional form of the relationship between employment and green ARRA. More specifically, interpreting these differences as average short-run or long-run effects assumes that employment trends (and pre-trends) across different commuting zones are affected by observable and unobservable covariates in a linear way. As such, the pre-trend in the effect of green ARRA accurately approximates the counterfactual employment dynamics conditional on all covariates, in commuting zones receiving a larger fraction of green ARRA. For instance, the amount of green ARRA received may be a function of the pre-existing size of the green economy or past government policies in each commuting zone.

As an alternative identification strategy, we exploit the well-known fact that ARRA spending was allocated according to formulas that were in use before the passage of the Recovery Act (see the discussion of Chodorow-Reich, 2018).[11] Importantly, the formulaic instrument has a typical shift-share structure used in the seminal literature on cross-sectional multipliers (e.g. Nakamura and Steinsson, 2014, Goldsmith-Pinkham et al., 2020). In previous studies, such instrument satisfies the exclusion restriction of affecting total employment only through ARRA spending because the main source of endogeneity was the local effect of the Great Recession.

Following these studies, we use an instrument that combines the initial "share" of EPA plus DOE spending in the CZ (over total DoE and EPA spending) with the green ARRA "shift". Such instrument adds an exogenous shock in green expenditures to areas that were already

[11] According to Conley and Dupor (2013), 2/3 of ARRA spending were allocated using such formulaic approach to privilege shovel-ready projects that have an immediate impact on the economy. For instance, spending in road construction, education and health were allocated by the Recovery Act using the formulas in place before the act (Wilson, 2012; Garin, 2018). An example for green ARRA are Energy Efficiency and Conservation Block Grants. This program was created by the Energy Independence and Security Act of 2007, which provided specific guidelines for distribution of funds. ARRA provided additional funding for this program and stipulated that the same formulas for eligibility in the 2007 Act be used (American Recovery and Reinvestment Act of 2009). However, many DOE ARRA projects supported new infrastructure, such as grid modernization, and do not appear to have been allocated formulaically.

receiving larger amount of green spending before ARRA.[12] Unfortunately, endogeneity of green ARRA is also related to the persistent effect of pre-ARRA green investments of both private and public institutions. Thus, this instrumental variable strategy is less effective in our case. Because such an instrument adds an exogenous shock in green expenditures to areas that were already receiving larger green investments before ARRA, we face a problem similar to that put forward by Jaeger et al. (2018), who note that a shift-share instrument conflates short- and long-term effects. We follow their suggestion and take a "share" far in the past (i.e. an average share of DoE plus EPA spending between 2003 and 2004), under the assumption that the effect of past spending gradually fades away and thus it is excludable from the second stage. Note that having a reliable measure of pre-ARRA green government spending would be the ideal solution to distinguish the additional contribution of green ARRA from that of past trends associated with pre-ARRA green spending. However, as explained in Appendix D, building an accurate measure of pre-ARRA green spending is difficult due to the lack of details in public spending data pre-ARRA.

Overall, both the IV and the OLS solution of the endogeneity problem rest upon the untestable assumption that the pre-crisis effect of green ARRA is a good estimate of the counterfactual employment growth, conditional on the covariates. However, while neither solution is perfect, comparing the OLS and the IV results can be very informative as each approach minimizes a different source of endogeneity. The IV mitigates endogeneity related to non-random assignment of green ARRA subsidies but it represents an upper bound, as it may capture the effect of past and present green ARRA on areas that were already on a green path, i.e. compliers in a

[12] The instrument of green ARRA reads as: $IV_i = \frac{DoE\ Pre\text{-}ARRA_{i,2003-04}}{DoE\ Pre\text{-}ARRA_{2003-04}} \times \frac{Green\ ARRA\ DoE}{Pop_{2008}} + \frac{EPA\ Pre\text{-}ARRA_{i,2003-04}}{EPA\ Pre\text{-}ARRA_{2003-04}} \times \frac{Green\ ARRA\ EPA}{Pop_{2008}}$, where total green ARRA EPA and DoE per capita is reallocated to CZs depending on their respective pre-ARRA shares of spending over the national total, i.e. $\frac{DoE\ Pre\text{-}ARRA_{i,2003-04}}{DoE\ Pre\text{-}ARRA_{2003-04}}$ and $\frac{EPA\ Pre\text{-}ARRA_{i,2003-04}}{EPA\ Pre\text{-}ARRA_{2003-04}}$.

LATE terminology (Imbens and Angrist, 1994). The OLS does the opposite: the effect should be smaller as it is the average of the "exogenous" shock on compliers and the "endogenous" shock on non-compliers, which is however less likely to conflate the effect of green ARRA with that of past green policies.

Finally, the estimates obtained from the above empirical strategy provide the average effect of green stimulus on total employment. To explore the mechanism through which green stimulus affects employment, we extend our analysis to test for heterogeneous impacts of green spending. We do this in three ways. First, we consider whether the existing skill composition in each commuting zone changes the effectiveness of green ARRA, focusing on the mediating effect of a pre-existing pool of workers with a high level of green skills. Second, we estimate separate models for different sectors and occupations, to ascertain whether there is heterogeneity across different types of workers. Finally, we assess the distributional effect of green ARRA spending by estimating the green ARRA impact for different broad groups of workers, such as manual labor. This exercise will indicate whether skill-biased shifts in labor demand induced by green ARRA create winners and losers in particular workers' categories.

V. Results

This section presents the main results of the paper. Table 2 highlights the main takeaways of our empirical evaluation of green ARRA spending for three dependent variables: total employment, green employment and manual employment, and the two alternative ways of modeling regional effects. We focus on green employment as it is the main channel through which the effect of green ARRA spending should take place (e.g., Vona et al., 2019).[13] We focus on manual labor employment for its importance in the debate on the distributional effects of trade and technology shocks (e.g., Autor et al., 2013; Acemoglu and Restrepo, 2020) and of the rise of populism in the US (e.g., Autor et al., 2020). The Table reports the point estimates of the green ARRA coefficients for the pre-ARRA period ($\hat{\beta}_{pre}$), the short-term ($\hat{\beta}_{short}$) and the long-term ($\hat{\beta}_{long}$). In addition, we present the effects of the green stimulus net of pre-trends: $\hat{\beta}_{long} - \hat{\beta}_{pre}$ and $\hat{\beta}_{short} - \hat{\beta}_{pre}$. These estimated differences have larger standard errors than each estimated coefficient, so we must sacrifice some precision to remove pre-trends. However, they are particularly relevant when pre-trends are an issue. Finally, the Table also reports the number of jobs created per millions of dollars spent for both the net ($\hat{\beta}_{long} - \hat{\beta}_{pre}$ and $\hat{\beta}_{short} - \hat{\beta}_{pre}$) and the gross ($\hat{\beta}_{short}\ and\ \hat{\beta}_{long}$) effects.[14]

Three findings stand out from this Table. First, for all three dependent variables green the effectiveness of green ARRA emerges only in the long-run with approximately 10.4 jobs created per 1 $ million spent. Second, effects on total employment (columns 1 and 4) are imprecisely estimated and less credible due to the presence of pre-trends, especially in the specification with state fixed effects. Third, effects on green employment (columns 2 and 4) and manual labor (columns 3 and 6) illustrate, respectively, the reshaping and distributional effect of green spending. Roughly speaking, we find that all jobs created are in manual labor positions, while more than 1/5 are green jobs. These findings are qualitatively confirmed in comprehensive robustness checks of

[13] Green employment is measured by reweighing occupational employment by the share of specific tasks in each occupation that O*NET defines as “green” (see Appendix A3 and Vona et al., 2018).
[14] Since the quantification of the number of jobs created is not straightforward as in related papers, we report in Appendix B the arithmetic to translate the estimated coefficients into number of jobs created.

Table 2 – Baseline results

	OLS, state fixed effects			OLS, census division fixed effects		
Dep var: Change in log employment (by type) per capita compared to 2008	Total employment	Green employment	Manual occupations	Total employment	Green employment	Manual occupations
Green ARRA per capita (log) x D2005_2007	0.0026***	0.00001	0.0008	0.0016	-0.0003	-0.0004
	(0.0009)	(0.0043)	(0.0027)	(0.0011)	(0.0042)	(0.0028)
Green ARRA per capita (log) x D2009_2012	0.0026***	0.0040	0.0057**	0.0017*	-0.0015	0.0033
	(0.0008)	(0.0039)	(0.0022)	(0.0009)	(0.0048)	(0.0029)
Green ARRA per capita (log) x D2013_2017	0.0045***	0.0120**	0.0108**	0.0039*	0.0083	0.0102
	(0.0016)	(0.0050)	(0.0046)	(0.0022)	(0.0060)	(0.0061)
Jobs created, $1 million green ARRA:						
Pre-ARRA (2005-2007)	11.53***	0	0.92	7.35	-0.07	-0.47
	(3.85)	(0.87)	(2.98)	(4.94)	(0.85)	(3.10)
Short-run (2009-2012)	11.15***	0.78	5.48**	7.42*	-0.3	3.2
	(3.29)	(0.76)	(2.10)	(3.95)	(0.92)	(2.77)
Long-run (2013-2017)	20.8***	2.66**	11.34**	18.03*	1.84	10.76
	(7.37)	(1.11)	(4.80)	(10.15)	(1.34)	(6.46)
Short-run - pre-ARRA	0.03	0.78	4.7	0.33	-0.24	3.61
	(3.49)	(1.49)	(3.39)	(4.05)	(1.58)	(3.84)
Long-run - pre-ARRA	8.92	2.66	10.48*	10.45	1.92	11.2*
	(8.02)	(1.83)	(5.46)	(9.46)	(1.97)	(6.46)
R squared	0.7672	0.4159	0.5749	0.6819	0.3336	0.4907
Observations	7631	7631	7631	7631	7631	7631

Notes: Regressions weighted by CZ population in 2008. Sample: 587 CZ with at least 25,000 residents in 2008. Year fixed effects and state (or census division) x period fixed effects included. Additional control variables (interacted with D2005_2007, D2009_2012 and D2013_2017 dummies): Vigintiles of non-green ARRA per capita, Share of empl with GGS>p75 (2005), Population 2008 (log), Income per capita (2005), Import penetration (year 2005), Pre trend (2000-2007) empl manufacturing / pop, Pre trend (2000-2007) employment tot / pop, Pre trend (2000-2007) empl constr / pop, Pre trend (2000-2007) empl extractive / pop, Pre trend (2000-2007) empl public sect / pop, Pre trend (2000-2007) unempl / pop, Pre trend (2000-2007) empl edu health / pop, Empl manuf (average 2006-2008) / pop, Empl constr (average 2006-2008) / pop, Empl extractive (average 2006-2008) / pop, Empl public sect (average 2006-2008) / pop, Unempl (average 2006-2008) / pop, Empl edu health (average 2006-2008) / pop, Shale gas extraction in CZ interacted with year dummies, Potential for wind energy interacted with year dummies, Potential for photovoltaic energy interacted with year dummies, Federal R&D lab, CZ hosts the state capital, Nonattainment CAA old standards, Nonattainment CAA new standards. Standard errors clustered by state in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 2 (see Appendix C), where we exclude areas with unbalanced characteristics, define green ARRA in different ways and group areas with similar non-green ARRA spending differently.

Table 2 also shows that how we model regional effects matters for the results on total employment. We face a trade-off between models with smaller pre-trends and models with greater efficiency. For total employment, we observe pre-trends when using state fixed effects (Column 1), but not when using Census division fixed effects (Column 4). A possible explanation is that many ARRA funds were allocated as block grants to states using pre-existing formulas, making the allocations to states are plausibly exogenous (e.g. Wilson, 2012). While this is less true of ARRA's green energy investments, there are still green programs such as the State Energy Program where funds were allocated to state governments. Any exogenous variation in the allocation of green ARRA across states that was present is not used for identification when including state fixed effects. Moreover, states have discretion as to how to allocate these block grants within the state. For instance, states could have prioritized allocating green ARRA block grant funds to more prosperous commuting zones with "shovel-ready" green projects. Our results suggest that such targeting of stimulus spending to well-performing areas by state governments may have been the case for green stimulus spending.

In contrast, we observe no pre-trends for green or manual employment. Thus, the credibility of the green ARRA impact on these two variables is not undermined by the presence of pre-trends. The estimated coefficients for the 2005-2007 period are not only insignificant, but also an order of magnitude smaller than for total employment. Moreover, while the magnitude of green ARRA's impact on green and manual employment is similar using either state or census division fixed effects, our estimates are more precise when using state fixed effects. Thus, moving forward, we focus on the results using state fixed effects when looking at green and manual employment, but emphasize the results using census division fixed effects for total employment.

Before diving into these results and into important extensions in greater details, it is worth to go back to the issue of the comparison between the OLS and the IV estimator. In Table 2, as in the rest of the paper, we choose the OLS as the preferred estimator. This choice is based on two arguments that are illustrated in the Appendix D for sake of space. First, the predictive power of the shift-share instrument is weak with an F-test of 10 (for census dummies) or even below (for state dummies, see Table D1). The weak instrument problem is consistent with the fact that DOE spending (the bulk of green spending) was redirected towards green programs. Second, compared to the OLS estimator, the IV overstates both the pre-trends for total employment ($\hat{\beta}_{pre}$, see Table D2) and the net long-term effect of green ARRA per capita ($\hat{\beta}_{long} - \hat{\beta}_{pre}$), which, as expected, is imprecisely estimated due to a weak instrument problem. Although the IV results are still informative, suggesting that the effect of green ARRA is highly heterogeneous and much stronger on compliers, they exacerbate the source of endogeneity associated with the presence of pre-trends.

The rest of this section is organized as follows. Subsection A presents more results on total employment. In subsection B, we show that the pre-existing level of green skills matters, while subsection C explore results by sector. Finally, subsection D explores some distributional implications by focusing on the effect of green ARRA on different occupations.

A. A Discussion of Total Employment Effects

Looking at the results on total employment more closely, Columns (1) and (4) of Table 2 show that the *gross* short-term effect $\hat{\beta}_{short}$ is positive and statistically different from zero, but the *net* short-term effect $\hat{\beta}_{short} - \hat{\beta}_{pre}$ becomes statistically indistinguishable from zero. In terms of gross job creation, $1million of green spending adds between 7.4 and 11.1 new jobs in the short-term, which is in the lower range of estimates of papers evaluating other programs of the Recovery Act (Chodorow-Reich, 2019).[15] Clearly, the net short-term effect cannot be used to give clear policy advice due to the presence of pre-trends. Since green spending was allocated to areas growing faster before the crisis, the absence of a net short-term effect can either reflect a fast convergence to a higher pre-crisis steady state (so it should be interpreted as evidence supporting the use of green spending to restart the economy) or the greater resilience of greener areas (so it should be interpreted as evidence of lack of additionality).

Similar considerations apply to the interpretation of the long-term effect, which is also contaminated by pre-trends. In this case, however, a net job creation effect seems to clearly emerge both in terms of size and statistical significance, although the difference $\hat{\beta}_{long} - \hat{\beta}_{pre}$ is still not precisely estimated. The implied net job creation effect for $1 million spent are 8.9 with state fixed effects and 10.4 with Census division fixed effects. The respective gross job creation effects are instead 18 and 20.8. These ranges perfectly overlap with the range of previous ARRA estimates presented in Chodorow-Reich (2019), making it difficult to rank green spending in comparison with alternative programs. However, the fact that jobs created are permanent is clearly a positive aspect of green spending. This conclusion is reinforced in Figure C1 in Appendix C where we allow all the coefficients of equation (1) to vary yearly. As the year-by-year results show that ARRA impacts are trending upwards after the crisis, $\hat{\beta}_{long}$ in our main specification is a conservative estimate of the long-term effect.

Regarding the explanations for a stronger long-run effect of green ARRA, the presence of administrative delays such as buy American guidelines, determining prevailing wages to comply

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[15] Note that other papers estimate gross job creation effects, while we privilege the hyper conservative estimation given by the net short-term effect. Other papers also use a formulaic IV that identifies the LATE effect of compliers, which is found to be generally larger than the effect on the entire population.

with the Davis-Bacon Act and complying with local regulations (Carley et al., 2014; Carley, 2016), seem unlikely to drive the high persistency of the green ARRA effect. At most, administrative delays can retard the effect of green ARRA for one or two years after 2012 (the last year when money was officially spent), but are unlikely to extend the impact until 2017. Another potential explanation is that government investments attracted additional private investments in green sectors (Mundaca and Ritcher, 2015). Many ARRA programs required matching funds from the private sector, and this was particularly true of Department of Energy projects (Council of Economic Advisors, 2010). Transforming to a greener economy was expected to support long-term economic growth (Aldy 2013).[16] Unexplored in previous literature is the role that pre-existing availability of green skills may play a role in shaping the effect of green ARRA. While we cannot discriminate between those explanations with our data, the next section explores the role of green skills in shaping the time profile of the green ARRA effect.

B. The Mediating Effect of Green Skills

In this section, we test if commuting zones with a workforce more prepared to perform green tasks are more likely to experience larger gains, both in the short- and in the long-term. Consoli et al. (2016) and Vona et al. (2018) show that the types of skills workers need to work in green jobs are different than the skills needed in rest of the economy, requiring more on-the-job training as well as engineering and technical competences. Looking at the heterogeneous effect with respect to the existing skill base of the workforce allows also to shed light on the large gap between the OLS and IV estimates, improving the interpretation of our results. Because the instrumental variable results highlights much larger effects on compliers, i.e. CZs already investing

[16] For example, the DOE's smart grid program invested $4.5 billion in new smart grid technology, which was matched by $6 billion in private sector funds. It is reasonable to expect such new infrastructure investment to provide lasting benefits for green employment.

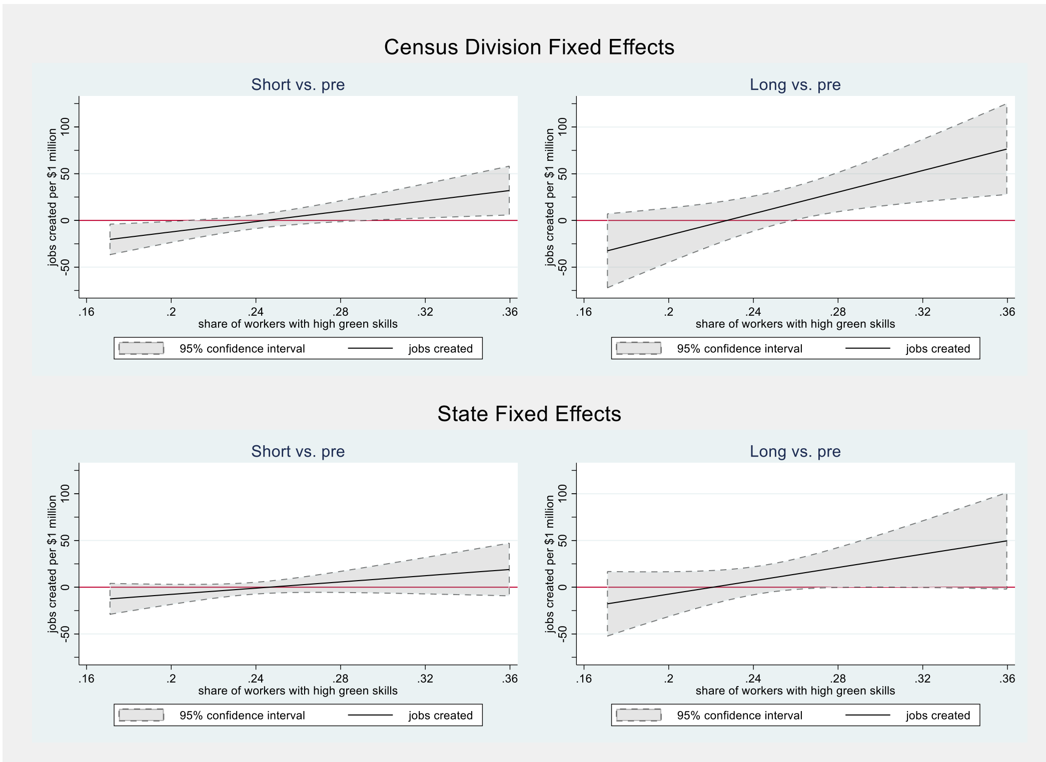
into the green economy, one might expect green stimulus to be more effective in areas with a higher concentration of green skills.

We use the data on green skills described in section III to identify the share of employment in each commuting zone in occupations with green skills importance in the 75th percentile or higher in 2006 (i.e. prior to the recession). While these jobs need not themselves be green, this captures the local endowment of the types of skills in high demand in a green economy.

We augment our baseline model, which already controls for the initial concentration of green skills in a region, by interacting our green ARRA variables (pre-, short- and long-) with the share of employment in occupations with green skills importance in the 75th percentile or higher. Recall that the initial concentration of green skills in a region is positively associated with the allocation of green ARRA spending.

Figure 6 shows the marginal effect of green ARRA net of the pre-trend at different levels of initial green skills for both the specification with state and census division dummies. Complete regression results are in Table C1 of Appendix C. The results show the importance of the initial skill base. The effect of green ARRA is significantly stronger in CZs with a higher concentration of green skills, particularly so in the specification with Census division dummies. As evident from Figure 6, the net short-term effect is increasing with the skill share, and becomes significant when the share of workers with high green skills is nearly 29.2 percent. To put this figure into context, such a share is in the 93rd percentile of all communities. The net long-term effect displays the same patterns, with statistically significant effect of green ARRA emerging when nearly 26 percent of workers have high green skills (66th percentile of all communities) when using census division fixed effects, and nearly 28.6 percent (91st percentile of all communities) in the most conservative specification with state fixed effects. These findings indicate that the availability of the right competences in loco is essential to both increase and accelerate the effect of green spending.

Figure 6 – Variation in the Effect of Green ARRA on employment by initial Green Skills



Notes: plot of the marginal effects of green ARRA, conditional on initial Green Skills. Calculations based on estimates from Appendix Table C1.

Figure 6 visually displays a large divergence in the magnitude of the effects across CZs with different initial level of GGS. More specifically, computations reported in the last rows of Appendix Table C1 show that, at the 75th percentile, 22.8 (16.4 with state dummies) jobs per $1 million are created in the long-run. In contrast, at the 25th percentile, we estimate an insignificant long-term effect of only 4.6 (5.2 with state dummies) jobs per $1 million. The top estimates are definitely in the upper bound of the range provided by Chodorow-Reich (2019) and are broadly consistent with the results of the IV pointing to much larger effects on compliers (Appendix D).

The result is even more remarkable by noting the fact that the initial share of occupations in the upper quartile of GGS importance itself has a large effect on future employment growth that is trending upwardly over time (Appendix Table C1).[17] Recall from Table 1 that the initial share of occupations in the upper quartile of GGS importance is also strongly correlated with the allocation of green ARRA subsidies. In combination, these results reinforce our interpretation of the green stimulus as a successful example of picking the winners. The main policy lesson is that increasing the green skills in a community should represent a key part of a successful policy package for the green transition as developing these skills will help other policies to work better.

C. Heterogeneous effects across sectors

In this section, we explore further how the green stimulus affects employment by considering heterogeneous effects across sectors. As the effect of the green stimulus is likely to be concentrated in certain sectors, our analysis sheds light on how green policies reshape the structure of the local economy. This exercise provides an initial account of the mechanics through which green ARRA stimulates employment and acts as a validation check that green ARRA really hits these target sectors.

Table 3 reports again the results on green employment and considers four additional sectors: manufacturing (NAICS 31-33), construction (NAICS 23), public administration (NAICS 92), and support services including waste management (NAICS 56). Those sectors are either most likely to receive green subsidies (e.g., construction and waste management) or to employ workers

[17] A one standard deviation in the green skills share (0.027) accounts, in the most conservative specification with state fixed effects, for a 0.97% difference in employment growth before the crisis that increases up to 1.91% in the short-term and 2.38% in the long-run.

needed to administer and monitor ARRA programs (e.g., public administration). We use the specification with state fixed effects here to increase precision in estimating net effects.[18]

Table 3 – Results by sector

Dep var: Change in log employment (by type) per capita compared to 2008	Green employment	Manufacturing sector (NAICS 31-33)	Construction sector (NAICS 23)	Support services including waste management (NAICS 56)	Public Sector employment
Green ARRA per capita (log) x D2005_2007	0.00001	0.0057***	-0.0017	-0.0063	0.0025
	(0.0043)	(0.0021)	(0.0032)	(0.0131)	(0.0037)
Green ARRA per capita (log) x D2009_2012	0.0040	0.0037**	0.0035	0.0136	-0.0148*
	(0.0039)	(0.0016)	(0.0032)	(0.0086)	(0.0075)
Green ARRA per capita (log) x D2013_2017	0.0120**	0.0069*	0.0143***	0.0063	-0.0133
	(0.0050)	(0.0040)	(0.0052)	(0.0097)	(0.0096)
Jobs created, $1 million green ARRA:					
Pre-ARRA (2005-2007)	0	2.86***	-0.43	-1.65	0.55
	(0.87)	(1.05)	(0.81)	(3.43)	(0.82)
Short-run (2009-2012)	0.78	1.54**	0.65	3.2	-3.37*
	(0.76)	(0.65)	(0.61)	(2.03)	(1.70)
Long-run (2013-2017)	2.66**	2.98*	3.02***	1.69	-2.94
	(1.11)	(1.73)	(1.10)	(2.61)	(2.13)
Short-run - pre-ARRA	0.78	-0.81	0.98	4.68*	-3.94
	(1.49)	(0.94)	(1.04)	(2.78)	(2.40)
Long-run - pre-ARRA	2.66	0.53	3.39**	3.39	-3.49
	(1.83)	(2.35)	(1.28)	(3.20)	(2.75)
R squared	0.4159	0.5514	0.7039	0.2345	0.3338
Observations	7631	7631	7631	7631	7631

Notes: OLS model weighted by CZ population in 2008. Sample: 587 CZ with at least 25,000 residents in 2008. Year fixed effects and state x period fixed effects included. Additional control variables (interacted with D2005_2007, D2009_2012 and D2013_2017 dummies) same as Table 2. Standard errors clustered by state in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

As shown earlier in Table 2, the green stimulus has a large long-term effect on green employment. While 4.6% of total employment is green, roughly 20 percent of the jobs created by green ARRA were green.[19] Both the pure long run and long-run additionality effect ($\hat{\beta}_{long} - \hat{\beta}_{pre}$)

[18] Note that looking at specific sectors we further loose precision in estimating net effects. Besides the fact that estimated net effects are noisier by construction, effects for specific sectors are more difficult to detect due to the larger dispersion of sectoral employment compared to total employment. To see this, the information in Table A.6 can be sued to compute the coefficients of variation for each dependent variable. These are always above 0.35 for different types of sectoral employment, but just 0.16 for total employment. State fixed effects reduce the noise of sectoral employment data compared to census division fixed effects.

[19] 4.6% is higher than the estimate of 3.1% provided by Vona et al. (2019) for 2014. This can be due to an aggregation bias or to the fact that we add three years after 2014. See Appendix A3 for greater details.

are large in absolute term with 2.7 green jobs created per \$1 million spent. The additionality effect appears statistically insignificant even though $\hat{\beta}_{pre}$ is zero and $\hat{\beta}_{long}$ is significant at 5% level just because the $\hat{\beta}_{long} - \hat{\beta}_{pre}$ effect captures the pure noise of the estimated $\hat{\beta}_{pre}$. This example illustrates the issue of statistical precision in estimating net effects.

The green stimulus also led to job creation in the construction sector. Of the 8.9 jobs created per \$1 million green ARRA in the long-term, about 40% (3.39) are in this sector. This is consistent with green ARRA targeting projects such as building renovation for energy efficiency or construction of renewable energy projects. Once again, pre-trends are less of concern in this sector, as the coefficients of $\hat{\beta}_{pre}$ are statistically insignificant.

The other three sectors were not significantly impacted by the green stimulus package, but for different reasons. While "support services including waste management" also accounts for slightly less than 40% of total job creation, both the net and the gross effects are far from being statistically significant, except for the short-run effect net of pre-trends, which is significant at the 10 percent level. In contrast, the lack of an additionality effect for manufacturing is associated with a positive pre-ARRA effect, meaning that green ARRA reinforced a pre-existing advantage in manufacturing. Finally, we find that green ARRA spending reduces the share of employment in the public sector, at least in the short-run. This result reassures us that the effect on total employment is not associated with a crowding out of private jobs.

Overall, the green stimulus reshaped labor markets by increasing the size of the local green economy as well as employment in construction and waste management. However, the distributional effect of the stimulus among workers is less clear. While greener tasks are concentrated in high-skills and thus well-paid occupations (Vona et al., 2019), construction and waste jobs may boost the creation of jobs that pay less. We explore this issue in the next section.

D. Distributional Effects of Green Stimulus

Our results for different sectors of the economy suggest that the green stimulus might have important distributional effects. In this section, we consider whether the effect of green stimulus varies for different types of workers. We estimate separate models for different broad groups of workers following a standard grouping in the literature on task-biased technological change (Acemoglu and Autor, 2011): abstract occupations, service workers, clerical occupations, and manual labor (see Table A5 in Appendix A3).

Table 4 shows results for these four occupational groups that were partly anticipated by the highlights presented in Table 2. The important result here is that all job creation from green ARRA occurs in manual labor occupations, while both the net and the gross effects for other occupational groups are far from being significant at conventional levels. To be more precise, the number of jobs created in manual positions per $1 million of green ARRA even exceeds the total number of jobs created in the long-run (10.45 vs. 8.95). Notably, the net effect on manual employment starts emerging in the short-term and is not contaminated by the presence of pre-ARRA trends. The short-run effect is smaller, however (only 4.7 jobs per $ 1 million of green ARRA).

Manual workers have been losing in terms of wages and employability for trade (e.g., Autor et al., 2013), automation (e.g., Acemoglu and Restrepo, 2020) and, but to a lesser extent, the effect of climate policies (e.g., Marin and Vona, 2019). It is thus important to provide an in-depth look at how the green stimulus affected manual labor. Table 5 considers the effect of green ARRA on manual labor wages (columns 1-3) and on educational attainment of manual workers. First, column 1 replaces changes in per capita employment as the dependent variable with the average hourly wage of manual workers. Despite increasing demand for manual labor, green ARRA investments

Table 4 – Results by occupational group

Dep var: Change in log employment (by occupational group) per capita compared to 2008	Manual occupations	Abstract occupations	Service occupations	Clerical occupations
Green ARRA per capita (log) x D2005_2007	0.0008	0.0036**	0.0025	0.0040*
	(0.0027)	(0.0017)	(0.0027)	(0.0022)
Green ARRA per capita (log) x D2009_2012	0.0057**	0.0006	-0.0017	-0.0005
	(0.0022)	(0.0020)	(0.0033)	(0.0026)
Green ARRA per capita (log) x D2013_2017	0.0108**	-0.0017	0.0001	0.0019
	(0.0046)	(0.0044)	(0.0041)	(0.0027)
Jobs created, $1 million green ARRA:				
Pre-ARRA (2005-2007)	0.92	5.28**	1.82	4.51*
	(2.98)	(2.47)	(1.97)	(2.49)
Short-run (2009-2012)	5.48**	0.98	-1.29	-0.51
	(2.10)	(3.07)	(2.53)	(2.75)
Long-run (2013-2017)	11.34**	-2.84	0.08	1.96
	(4.80)	(7.24)	(3.36)	(2.84)
Short-run - pre-ARRA	4.7	-4.43	-3.22	-4.69
	(3.39)	(5.12)	(4.16)	(4.75)
Long-run - pre-ARRA	10.48*	-8.79	-1.99	-2.24
	(5.46)	(8.53)	(4.84)	(4.69)
R squared	0.5749	0.5846	0.4747	0.4112
Observations	7631	7631	7631	7631

Notes: OLS model weighted by CZ population in 2008. Sample: 587 CZ with at least 25,000 residents in 2008. Year fixed effects and state x period fixed effects included. Additional control variables (interacted with D2005_2007, D2009_2012 and D2013_2017 dummies) same as Table 2. Standard errors clustered by state in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 5 – Focus on manual occupations

Dep var: Change in log employment (by category) per capita compared to 2008 (except column 1)	Average hourly wage of manual workers	Manual workers, hourly wage > US med. for manual workers	Manual workers, hourly wage < US med. for manual workers	Manual workers with education > high school degree	Manual workers with high school degree or less
Green ARRA per capita (log)	0.0052	0.0016	-0.0007	-0.0028	0.0024
x D2005_2007	(0.0049)	(0.0042)	(0.0028)	(0.0046)	(0.0030)
Green ARRA per capita (log)	-0.0029	0.0046	0.0088***	0.0117***	0.0038
x D2009_2012	(0.0047)	(0.0032)	(0.0027)	(0.0043)	(0.0028)
Green ARRA per capita (log)	0.0022	0.0099*	0.0123**	0.0121**	0.0096*
x D2013_2017	(0.0055)	(0.0058)	(0.0049)	(0.0052)	(0.0053)
Jobs created, $1 million green ARRA:					
Pre-ARRA (2005-2007)	N/A	0.95	-0.35	-0.81	2.01
		(2.50)	(1.50)	(1.34)	(2.47)
Short-run (2009-2012)	N/A	2.34	4.01***	3.23***	2.61
		(1.63)	(1.25)	(1.19)	(1.91)
Long-run (2013-2017)	N/A	5.61*	6.01**	3.83**	7.12*
		(3.27)	(2.38)	(1.64)	(3.89)
Short-run - pre-ARRA	N/A	1.53	4.31**	4**	0.95
		(3.31)	(1.93)	(1.96)	(3.24)
Long-run - pre-ARRA	N/A	4.71	6.34**	4.71*	5.34
		(4.08)	(3.14)	(2.53)	(4.71)
R squared	0.3760	0.4825	0.4949	0.3488	0.5546
Observations	7631	7631	7631	7631	7631

Notes: OLS model weighted by CZ population in 2008. Sample: CZ with at least 25,000 residents in 2008. Year fixed effects and state x period fixed effects included. Additional control variables (interacted with D2005_2007, D2009_2012 and D2013_2017 dummies) same as Table 2. Standard errors clustered by state in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

did not increase the wages of manual workers.[20] In columns (2) and (3), we see that most of the increase in manual labor jobs occurred in jobs where workers earned less than the US median wage for all manual workers. This missing wage gains highlight the well-known deterioration of the bargaining power of manual workers that requires other solutions than public spending in the green economy. While the manual labor jobs created by green ARRA were not high-paying jobs, they are not necessarily low skilled jobs. In the last two columns, we see that much of the increase in manual labor work is among manual workers who have more than a high-school education. In fact, this group of workers experiences job gains from green ARRA investments in both the short term (4 jobs per $1 million) and long term (4.71 jobs per $1 million). While the green stimulus increased demand for manual labor workers, these jobs still required higher education and were not better paying than existing jobs.

VI. Discussion

We perform a comprehensive evaluation of the economic effect of green stimulus using the historical experience of the American Recovery and Reinvestment Act, which represents the largest push to the green economy to date. Our results inform both current policy debates and address longer-term concerns about job losses in the transition to a green economy. Currently, some environmentalists advocate green new deal programs as a win-win solution to both relaunch sluggish economic growth in developed countries and to tackle climate change. The Covid-19 lockdown has led to calls for large-scale investments in the green economy. While the size of the green stimulus of 2009 is small compared to what is at stake for a post-Covid-19 recovery, our

[20] This may be explained by the need to comply with prevailing wage laws. Since contractors were required to document that workers were paid prevailing wages, they had little incentive to pay more than the prevailing wage. We thank Joe Aldy for this insight.

research highlights interesting features of a green stimulus that can offer guidance to the design of future green stimulus programs.

First, our results suggest green ARRA works more slowly than other stimulus investments. The long-run effect of green ARRA on total employment is in the mid-range of previous estimates, with just over 10 jobs created per $1 million of green ARRA. The persistency of the job creation effect is clearly a positive aspect of the green fiscal stimulus. However, the timing of green ARRA's impact differs from previous studies of other ARRA investments, which generally find short-term effects. For green ARRA, we do not find evidence of short-run employment gains. The timing of green stimulus investments has two implications. First, green stimulus investments appear more effective for reshaping an economy than for restarting an economy. While our focus is on the potential employment benefits from green investments, future research should also consider the potential environmental benefits of green stimulus, as the long-run impacts on employment suggest that green investments lead to durable changes in the green economy. Second, while beyond the scope of this analysis, it may be that green stimulus investments need to be combined with other standard short-term responses, such as extensions to unemployment benefits and financial support to business, to provide immediate impact.

Second, the impact of the green stimulus becomes much clearer when we explore several dimensions of heterogeneity. Green ARRA creates more jobs in commuting zones with larger initial shares of occupations that use intensively such skills. In particular, $1 million of green ARRA spending creates approximately twice as many jobs in areas in top quartile of the green skills distribution than in the average commuting zone. The bottom line is that the green stimulus has been particularly effective in picking winners – e.g. enhancing opportunities in communities already in position to support a green economy. Care must be taken to match green investments to the skill base of the local economy. In light of our results, the green ARRA stimulus probably devoted too little resources (less than 1%) to on-the-job training. To support communities without the required green skills, expanding specific technical programs and engineering education (the most important green skills) could complement green stimulus investments. Evaluation of such training programs is left for future work.

Third, a green stimulus has potential to reshape an economy and thus may have important distributional effects. Green ARRA increases the demand especially for manual laborers. Importantly, pre-trends are not an issue when we study how the effect of green ARRA varies across sectors and occupations. Beyond the direct impacts of a green stimulus, these results also have broader implications for whether governments can help ease labor market transitions in response to environmental policy. Recent studies suggest that environmental regulation may reduce jobs in specific sectors, particularly for lower skilled manual labor (Marin and Vona, 2019; Yip, 2019). In contrast, subsidies to green infrastructure can benefit unskilled workers and thus may enhance the political support for other climate policies. However, wage gains did not follow the increase in the demand of manual tasks in areas receiving higher green subsidies. Exploring whether this is due to the fact that green jobs in construction are of low quality compared to similar jobs, or to the widespread deterioration of employment opportunities of the unskilled requires the use of longitudinal worker-level data and is left for future research.

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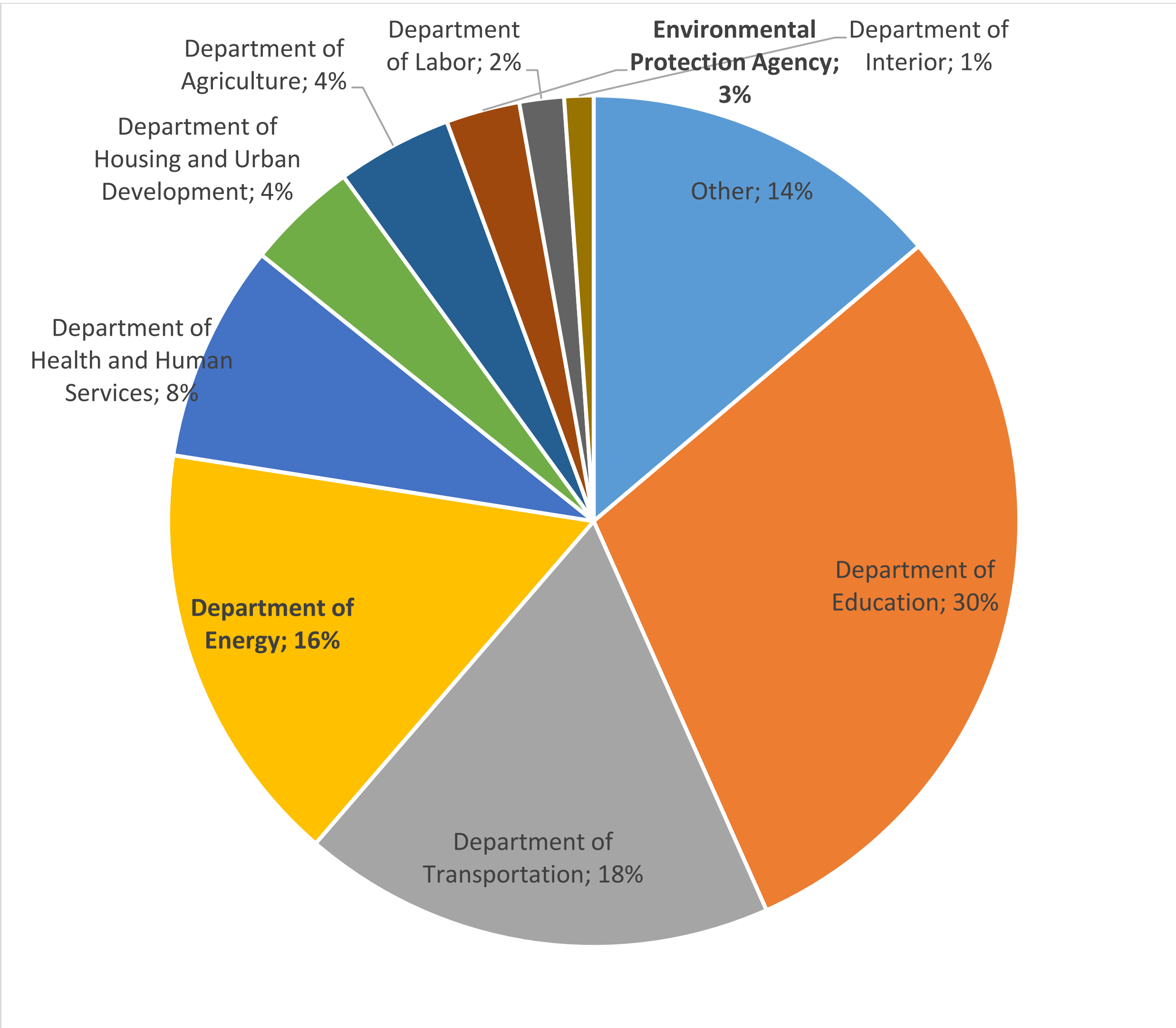
*** <u>APPENDICES FOR ON-LINE PUBLICATION ONLY</u> ***

Appendix A - Data Appendix

A1 – Background on Green ARRA investments

Figure A1 – ARRA spending by awarding Department / Agency



Notes: own elaboration based on Recovery.gov data from NBER data repository.

Table A1 – Descriptive statistics for green and non-green ARRA

	Non-green ARRA	Green ARRA	DOE ARRA	EPA ARRA	Green research ARRA	Green training ARRA
Total, million $	261,667	61,193	52,134	9,059	6,191	228
By commuting zone, million $						
mean	440.14	103.39	88.16	15.23	10.55	0.39
s.d.	985.26	308.60	294.26	28.99	70.21	1.38
min	1.59	0.00	0.00	0.00	0.00	0.00
median	143.45	18.27	10.19	6.07	0.00	0.00
max	9,931.67	3,677.57	3,601.58	297.57	1,163.62	11.96
By commuting zone, per capita						
mean	985.20	260.39	213.04	47.35	23.70	0.67
s.d.	630.11	1,303.28	1,298.28	65.82	313.19	3.83
min	8.65	0.00	0.00	0.00	0.00	0.00
median	818.96	104.67	57.71	27.40	0.00	0.00
max	6,788.70	28,398.38	28,292.04	640.88	7,377.34	70.33

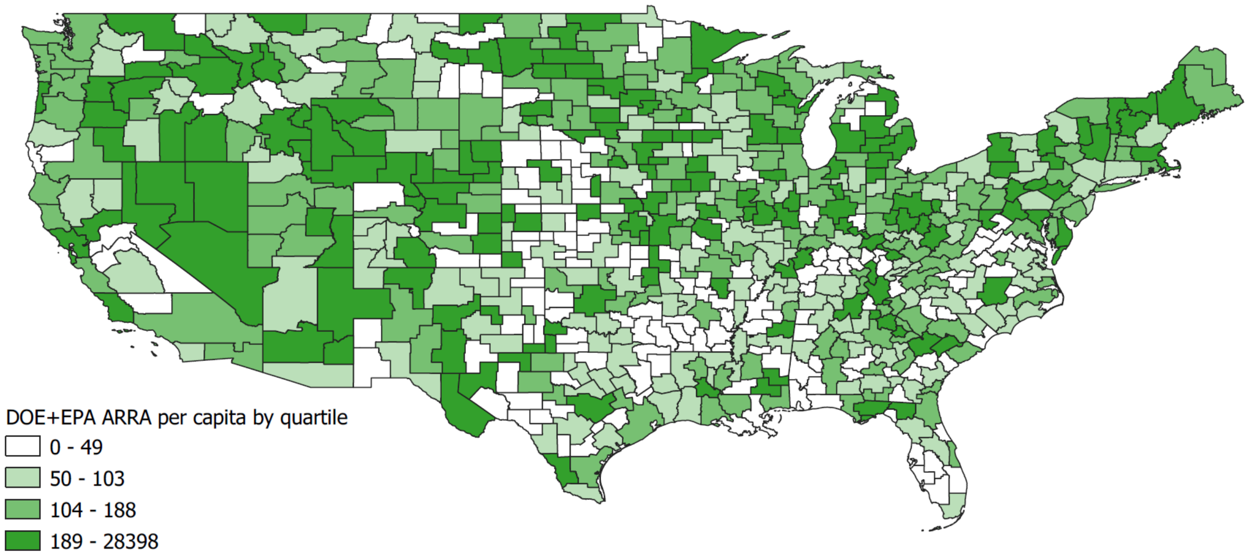
Notes: data by 587 commuting zone includes only CZ with at least 25000 inhabitants. ARRA for years 2009-2012 divided by population in 2008 (dollars per capita).

Table A2 – Top 10 areas in terms of green and non-green ARRA per capita

Top 10 CZ by green ARRA per capita			
Main county of the CZ	Green ARRA per capita	Non-green ARRA per capita	Population in 2008
Morgan County, IL	28398	1163	55090
Orangeburg County, SC	8283	1028	157729
Benton County, WA	6754	599	298566
Elko County, NV	5722	1098	59144
Alamosa County, CO	4130	1711	45845
Lee County, MS	3031	1089	204392
Frederick County, MD	2856	1037	709225
Santa Barbara County, CA	2313	712	682217
Knox County, TN	2294	921	849156
Larimer County, CO	1839	1475	291650
Top 10 CZ by non-green ARRA per capita			
Main county of the CZ	Non-green ARRA per capita	Green ARRA per capita	Population in 2008
Sangamon County, IL	6789	291	321216
Fairbanks North Star Borough, AK	4905	185	101940
Clarke County, IA	3978	330	33184
Leon County, FL	3922	456	383912
Union County, IA	3641	136	28110
Stutsman County, ND	3565	760	34258
Bell County, TX	3509	59	398202
Montgomery County, KY	1397	127	116545
Morgan County, GA	3169	125	54433
Riley County, KS	3081	124	135221

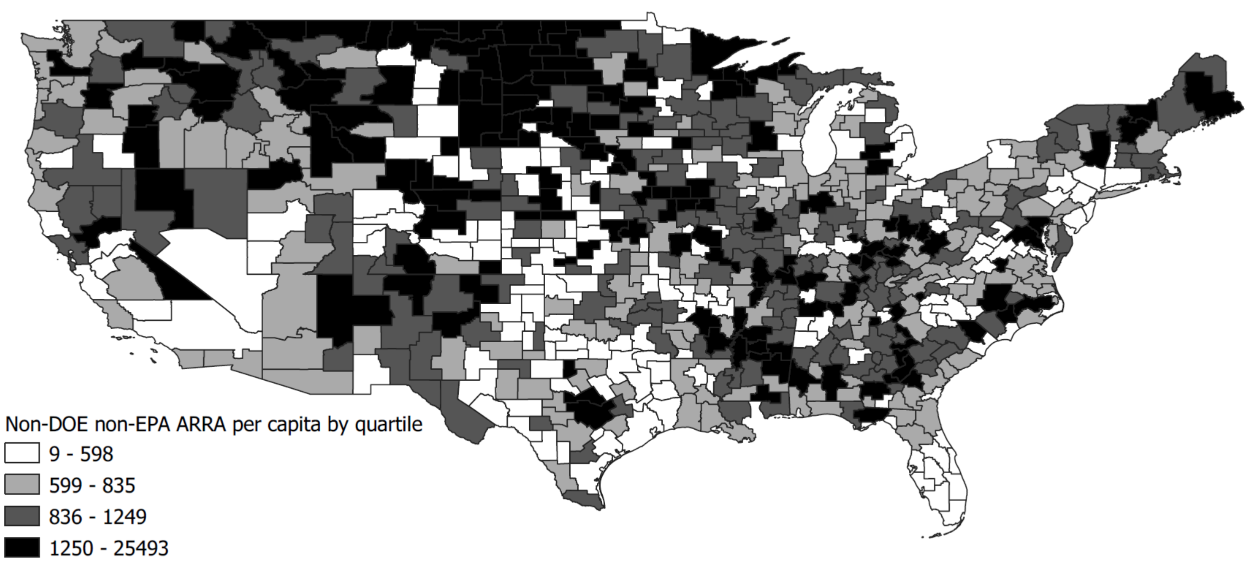
Notes: only CZ with at least 25000 inhabitants. ARRA for years 2009-2012 divided by population in 2008 (dollars per capita). Main county of the CZ identified as the county with the largest population level.

Figure A2 – Green ARRA spending per capita by Commuting Zone



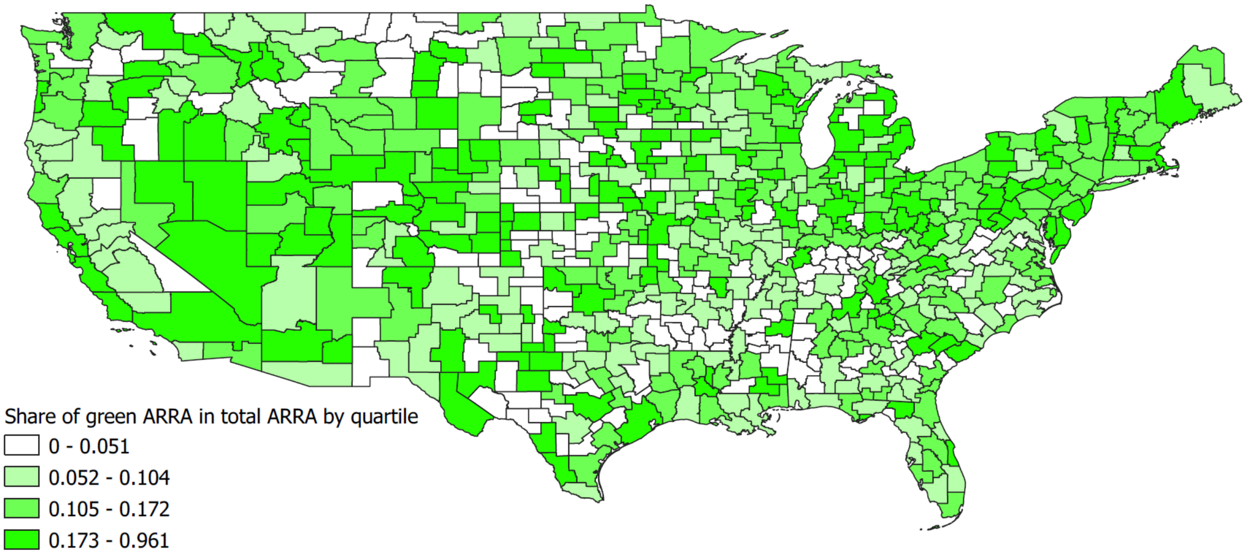
Notes: own elaboration based on Recovery.gov data from NBER data repository. Green ARRA is defined as ARRA spending awarded by DOE and EPA broken down by quartiles. Per capita analysis based on the population of each commuting zone prior to the recession, in 2008. Alaska and Hawaii not shown.

Figure A3 – Non-green ARRA spending per capita by Commuting Zone



Notes: own elaboration based on Recovery.gov data from NBER data repository. Non-green ARRA is defined as ARRA spending awarded by all agencies except DOE and EPA broken down by quartiles. Per capita analysis based on the population of each commuting zone prior to the recession, in 2008. Alaska and Hawaii not shown.

Figure A4 – Share of green ARRA in total ARRA spending by Commuting Zone



Notes: own calculation based on Recovery.gov data from NBER data repository. Green ARRA is defined as ARRA spending awarded by the DOE and EPA. Each shade represents a different quartile. Alaska and Hawaii not shown.

A2 - Control variables: definitions and data sources

Data on average annual employment level by county and year is retrieved from the BLS-QCEW (Quarterly Census of Employment and Wages of the Bureau of Labor Statistics). County-level data are then aggregated up at the CZ level. We use BLS-QCEW also to estimate employment by industry. In all regressions, we account for the base-year (2008) level of CZ employment per capita by industry as well as the growth in CZ employment per capita (population in 2008) by industry and total over the period 2000-2007 (pre-trends).

Data on unemployed persons is obtained from the BLS-LAUS Local Unemployment Statistics database while data on county-level population and personal income per capita is retrieved from the database maintained by the Bureau of Economic Analysis.

Data on occupations and skills are based on microdata from the Decennial Census (5% sample, year 2000) and the American Community Survey (ACS, 1% sample of the US population, years 2005-2017) available at IPUMS (Integrated Public Use Microdata Series, Ruggles et al., 2020). We just consider working-age (16-64) employed persons. We allocate worker-level information to CZs based on the worker's place of work (county place of work: 59.2% of workers; PUMA place of work: 32.5% of workers) and, when not available, county of residence (8.3% of workers). Based on the definition of commuting zone, most of these residual workers should be employed within the same CZ where they reside.

As described briefly in Section III.A of the paper, we use ACS microdata to build our indicator of GGS endowment. For all 448 SOC-based occupations, we compute for years 2000 (Decennial Census) and 2005 (ACS) the average importance score of Green General Skills (GGS, see Vona et al., 2018) using data on tasks and skills from the O*NET (Occupational Information Network) database (version: 18.0). Based on the national cross-occupation weighted (by sample weights times hours worked) distribution of GGS importance scores in 2000, we compute the 75th percentile of the distribution. Then, using data from ACS for 2005, we compute the share of hours worked by employees in each CZ in occupations above the threshold of GGS (see Table A3) over total hours worked by employees in each CZ.

Table A3 – List of occupations in the top quartile of GGS

SOC code	Occupation title
111021	General and Operations Managers
113051	Industrial Production Managers
113061	Purchasing Managers
119021	Constructions Managers
119111	Medical and Health Services Managers
119121	Natural Science Managers
131023	Purchasing Agents, Except Wholesale, Retail, and Farm Products
131051	Cost Estimators
131081	Logisticians
132099	Financial Specialists, All Other
171010	Architects, Except Naval
171020	Surveyors, Cartographers, and Photogrammetrists
172011	Aerospace Engineers
172041	Chemical Engineers
172051	Civil Engineers
172061	Computer Hardware Engineers
172070	Electrical and Electronics Engineers
172081	Environmental Engineers
172110	Industrial Engineers, including Health and Safety
172121	Marine Engineers and Naval Architects
172131	Materials Engineers
172141	Mechanical Engineers
173010	Drafters
173020	Engineering Technicians, Except Drafters
173031	Surveying and Mapping Technicians
191010	Agricultural and Food Scientists
191020	Biological Scientists
191030	Conservation Scientists and Foresters
192010	Astronomers and Physicists
192021	Atmospheric and Space Scientists
192030	Chemists and Materials Scientists
192040	Environmental Scientists and Geoscientists
192099	Physical Scientists, All Other
193051	Urban and Regional Planners
2590XX	Other Education, Training, and Library Workers
291011	Chiropractors
291020	Dentists
291031	Dieticians and Nutritionists
291041	Optometrists
291051	Pharmacists
291060	Physicians and Surgeons
291071	Physician Assistants
291081	Podiatrists

SOC code	Occupation title
291123	Physical Therapists
291124	Radiation Therapists
291126	Respiratory Therapists
291131	Veterinarians
291181	Audiologists
292010	Clinical Laboratory Technologists and Technicians
292030	Diagnostic Related Technologists and Technicians
292041	Emergency Medical Technicians and Paramedics
299000	Other Healthcare Practitioners and Technical Occupations
331012	First-Line Supervisors of Police and Detectives
331021	First-Line Supervisors of Fire Fighting and Prevention Workers
331099	First-Line Supervisors of Protective Service Workers, All Other
332011	Firefighters
332020	Fire Inspectors
333021	Detectives and Criminal Investigators
371012	First-Line Supervisors of Landscaping, Lawn Service, & Groundskeeping Workers
372021	Pest Control Workers
413099	Sales Representatives, Services, All Other
419031	Sales Engineers
452011	Agricultural Inspectors
454011	Forest and Conservation Workers
471011	First-Line Supervisors of Construction Trades and Extraction Workers
472011	Boilermakers
472111	Electricians
472150	Pipelayers, Plumbers, Pipefitters, and Steamfitters
472211	Sheet Metal Workers
474011	Construction and Building Inspectors
474021	Elevator Installers and Repairers
474041	Hazardous Materials Removal Workers
474051	Highway Maintenance Workers
475031	Explosives Workers, Ordnance Handling Experts, and Blasters
475040	Mining Machine Operators
491011	First-Line Supervisors of Mechanics, Installers, and Repairers
493011	Aircraft Mechanics and Service Technicians
499021	Heating, Air Conditioning, and Refrigeration Mechanics and Installers
499044	Millwrights
49904X	Industrial and Refractory Machinery Mechanic
499051	Electrical Power-Line Installers and Repairers
499094	Locksmiths and Safe Repairers
518010	Power Plant Operators, Distributors, and Dispatchers
518021	Stationary Engineers and Boiler Operators
518031	Water and Wastewater Treatment Plant and System Operators
518090	Miscellaneous Plant and System Operators
532010	Aircraft Pilots and Flight Engineers
536051	Transportation Inspectors
1110XX	Chief Executives and Legislators
119013	Farmers, Ranchers, and Other Agricultural Managers
119041	Architectural and Engineering Managers
119199	Funeral Directors
119XXX	Miscellaneous Managers, Including Funeral Service Managers and Postmasters and Mail Superintendents
131041	Compliance Officers, Except Agriculture, Construction, Health and SAfety, and Transportation
151111	Computer Scientists and Systems Analysts
151121	Computer and Information Research Scientists
151122	Information Security Analysts
151143	Computer Network Architects
1720XX	Biomedical and agricultural engineers
1721XX	Petroleum, mining and geological engineers, including mining safety engineers
1721YY	Miscellaneous engineeers including nuclear engineers

SOC code	Occupation title
1910XX	Medical Scientists, and Life Scientists, All Other
1930XX	Miscellaneous Social Scientists, Including Survey Researchers and Sociologists
1940YY	Miscellaneous Life, Physical, and Social Science Technicians, Including Research Assistants
2310XX	Lawyers, and judges, magistrates, and other judicial workers
29112X	Other Therapists, Including Exercise Physiologists
451011	First-Line Supervisors of farming, fishing, and forestry workers
472XXX	Miscellaneous construction workers including solar Photovaltaic Installers, and septic tank servicers and sewer pipe cleaners
49209X	Electrical and electronics repairers, transportation equipment, and industrial and utility
49909X	Other Installation, Maintenance, and Repair Workers
5360XX	Miscellaneous transportation workers including bridge and lock tenders and traffic technicians
5370XX	Conveyor operators and tenders, and hoist and winch operators
537XXX	Miscellaneous Material Moving Workers

To calculate import penetration, we begin with data at the US-level (year 2005). We compute sector-specific (4-digit NAICS) import penetration as the ratio between total import of manufactured products of each sector and total 'domestic use' of products of the same sector (import + domestic output – export). Data on import and export by sector are retrieved from Schott (2008), while domestic output is retrieved from the NBER-CES database. We then estimate CZ-level import penetration as the weighted average of sector-specific (4-digit NAICS) national import penetration, using employment by CZ and 4-digit NAICS sector as weights (source: County Business Patterns database).

To account for the presence of shale gas extraction, we obtained geospatial data on shale gas and oil play boundaries from the US Energy Information Administration.[21] We use GIS to compute a dummy variable equal to 1 if the CZ overlaps any of the shale oil and gas resources. Thus, the indicator represents the *potential* for shale oil or gas activity. To avoid endogeneity, we do not include actual drilling activity.

Indicators of wind and photovoltaic energy potential are based on detailed information from the National Renewable Energy Laboratory.[22] For wind, this information includes speed and

[21] https://www.eia.gov/maps/maps.htm, last accessed May 27, 2020.
[22] https://www.nrel.gov/gis/index.html, last accessed May 27, 2020.

variability of winds at different heights and for the presence of obstacles. For solar, this information considers the intensity and slope of solar radiation and for obstacles and terrain slope. We attribute to each CZ the average indicator of potential for wind and photovoltaic energy generation, ranging from 1 (low potential) to 7 (high potential).

We compute two dummy variables to account for the presence of local stringent environmental regulation to limit air pollution within the Clean Air Act. The dummy variable NA CAA old standard is set to one if at least 1/3 of the CZ resides in counties that were designed as nonattainment according to National Ambient Air Quality Standards (NAAQS) set in the pre-sample period: carbon oxide (1971), lead (1978), NO2 (1971), ozone (1979; 1997), particulate matter <10 micron (1987), particulate matter <2.5 micron (1997), SO2 (1971). The dummy variable NA CAA new standards, instead, considers recently approved more stringent NAAQS: lead (2008), ozone (2008), particulate matter <2.5 micron (2006), SO2 (2010).

Finally, we manually detect the presence of Federal R&D laboratories and state capitals in each CZ and create two dummy variables.

Table A4 reports descriptive statistics, weighted by population in 2008, for all our control variables.

Table A4 – Descriptive statistics of control variables

Variable	mean	s.d.	min	median	max
Share of empl with GGS>p75 (year 2006)	0.251	0.027	0.171	0.251	0.360
Population 2008 (log)	14.197	1.423	10.136	14.377	16.685
Income per capita (2005)	38.149	8.067	18.229	37.815	77.863
Import penetration (year 2005)	0.008	0.005	0.001	0.006	0.051
Pre trend (2000-2007) employment tot / pop	-0.010	0.020	-0.092	-0.010	0.112
Pre trend (2000-2007) empl manufacturing / pop	-0.015	0.010	-0.090	-0.015	0.031
Pre trend (2000-2007) empl constr / pop	0.002	0.004	-0.013	0.001	0.027
Pre trend (2000-2007) empl extractive / pop	0.001	0.003	-0.009	0.000	0.101
Pre trend (2000-2007) empl public sect / pop	0.000	0.004	-0.046	0.000	0.057
Pre trend (2000-2007) unempl / pop	0.003	0.005	-0.016	0.003	0.021
Pre trend (2000-2007) empl edu health / pop	0.012	0.010	-0.039	0.011	0.068
Empl manuf (average 2006-2008) / pop	0.045	0.023	0.000	0.044	0.173
Empl constr (average 2006-2008) / pop	0.023	0.007	0.001	0.022	0.088
Empl extractive (average 2006-2008) / pop	0.002	0.006	0	0.000	0.148
Empl public sect (average 2006-2008) / pop	0.022	0.011	0.000	0.020	0.138
Empl edu health (average 2006-2008) / pop	0.072	0.022	0.001	0.071	0.169
Unempl (average 2006-2008) / pop	0.025	0.005	0.001	0.025	0.071
Shale gas extraction in CZ	0.343	0.475	0	0	1
Potential for wind energy	1.620	0.639	1	2	5
Potential for photovoltaic energy	5.083	0.832	4	5	7
Federal R&D lab	0.258	0.438	0	0	1
CZ hosts the state capital	0.222	0.415	0	0	1
Nonattainment CAA old standards	0.694	0.461	0	1	1
Nonattainment CAA new standards	0.365	0.481	0	0	1

Notes: data by commuting zone includes only CZ with at least 25000 inhabitants. Statistics weighted by population in 2008.

A3 - Dependent variables: definitions and data sources

Our main dependent variable is the change in total employment per capita (using population in 2008) compared to the base year 2008. Data on average annual employment level by county is retrieved from the BLS-QCEW (Quarterly Census of Employment and Wages of the Bureau of Labor Statistics). County-level data are then aggregated up at the CZ level. We also use BLS-QCEW to estimate employment by industry (columns 2-5 of Table 3).

Our measure of green employment (column 1 of Table 3) is estimated as:

$$Green\ emp_{i,t} = Greenness_o \times Share\_h\_worked_{o,i,t} \times TotEmp_{i,t}$$

where:

- $Greenness_o$ is computed as the importance-weighted share of green specific tasks over total specific tasks (source: O*NET, version 18.0) in occupation *o* as in Vona et al. (2019);
- $Share\_h\_worked_{o,i,t}$ is the share of hours worked by employees in SOC occupation *o* in CZ *i* and year *t* (source: IPUMS-ACS);
- $TotEmp_{i,t}$ is total employment in CZ *i* and year *t* (source: BLS-QCEW).

Our estimate of green employment is found to be, on average, an upper-bound compared to recent figures due to possible aggregation bias at the occupational level and to the fact that we consider three additional years (2015-2016-2017). Our benchmark is Vona et al. (2019), who estimate green employment using data on 'pure' 6-digit SOC occupational classification (775 occupations) from BLS-OES at the metropolitan and nonmetropolitan area level. According to their estimate, green employment accounts for 3% of total US employment in 2006-2014. Our estimates here, which use 448 occupations in IPUMS-ACS data by commuting zone, suggest that green employment is 4.6% of total US employment over a similar but slightly longer timeframe.

An example to illustrate the possible aggregation bias is the following. In ACS the occupation "17-3020 Engineering Technicians, Except Drafters" is not broken down into its 8 6-digit occupations. While the average greenness of 17-3020 is 0.16, it includes both 6-digit occupations with zero greenness (e.g. "17-3021 Aerospace Engineering and Operations Technicians") and occupations with greenness equal to one (e.g. "17-3025 Environmental Engineering Technicians"). Clearly, taking the unweighted average, as we did here, over-estimate the weight given to green occupations that taking the weighted average, as in Vona et al. (2019) whereby BLS data are available at a more disaggregated level from BLS-OES at the metropolitan and nonmetropolitan area level. The simple reason for this is that the relative size of green occupations within a broad category such as “17-3020 Engineering Technicians, Except Drafters” is smaller than the uniform weights that one would attribute in absence of employment statistics at a more disaggregated level. We refer the interested reader to Vona et al. (2019) for further evidence and discussions of the aggregation bias associated with the use of too coarse occupation-based measure of green employment.

Occupational groups (Table 4) are identified following the definition provided by Acemoglu and Autor (2011). The list of SOC occupations (ACS definition) by each macro occupational group is reported in Table A5. Similarly to the measure of greenness, we compute the share of hours worked (weighted by sampling weights) by employees in each macro-occupational group and CZ over the total hours worked in the CZ using data from IPUMS-ACS. The number of employees by occupational group is then computed as the product between the share of hours worked in CZ and the total number of employees (BLS-QCEW).

In our focus on manual occupations (Table 5), we identify sub-categories of manual workers based on data from IPUMS-ACS. We compute the hourly wage (column 1) as the ratio between total wages received and total annual hours worked. In column 2 and 3 we use, respectively, the share of manual workers with hourly wage above or below US-median hourly wage in the US. Finally, in columns 4 and 5 we consider the educational attainment of manual workers using information on educational attainment from IPUMS-ACS: we define manual workers with high school degree or more as those manual workers that completed at least the 12th grade.

Table A5 – Macro-occupational groups based on Acemoglu and Autor (2011) (definitions for SOC codes can be found at https://usa.ipums.org/usa-action/variables/OCCSOC#codes_section)

Macro-occupational group	SOC codes
Abstract occupations	111021, 1110XX, 112011, 112020, 112031, 113011, 113021, 113031, 113040, 113051, 113061, 119013, 119021, 119030, 119041, 119051, 119071, 119081, 119111, 119121, 119141, 119151, 119199, 119XXX, 131011, 131021, 131022, 131023, 131041, 131051, 131070, 131081, 131111, 131121, 131XXX, 132011, 132031, 132041, 132051, 132052, 132053, 132061, 132070, 132081, 132082, 132099, 151111, 151121, 151122, 151131, 151134, 15113X, 151141, 151142, 151143, 151150, 151199, 152011, 152031, 1520XX, 171010, 171020, 172011, 172041, 172051, 172061, 172070, 172081 ,1720XX, 172110, 172121, 172131, 172141, 1721XX, 1721YY, 173010, 173020, 173031, 191010, 191020, 191030, 1910XX, 192010, 192021, 192030, 192040, 192099, 193011, 193030, 193051, 1930XX, 194011, 194021, 194031, 1940YY, 2310XX, 232011, 232090, 251000, 252010, 252020, 252030, 252050, 253000, 254010, 254021, 259041, 2590XX, 271010, 271020, 272011, 272012, 272020, 272030, 272040, 272099, 273010, 273020, 273031, 273041, 273042, 273043, 273090, 274021, 274030, 2740XX, 291011, 291020, 291031, 291041, 291051, 291060, 291071, 291081, 291122, 291123, 291124, 291125, 291126, 291127, 29112X, 291131, 291181, 291199, 292010, 292021, 292030, 292041, 292050, 292061, 292071, 292081, 292090, 299000, 312010, 312020, 33909X, 391010, 519080, 532010, 532020
Manual occupations	471011, 472011, 472031, 472040, 472050, 472061, 472071, 47207X, 472080, 472111, 472121, 472130, 472140, 472150, 472161, 472181, 472211, 472XXX, 473010, 474011, 474021, 474031, 474041, 474051, 474061, 475021, 475031, 475040, 4750XX, 4750YY, 47XXXX, 491011, 492011, 492020, 492091, 492092, 492096, 492097, 492098, 49209X, 493011, 493021, 493022, 493023, 493031, 493040, 493050, 493090, 499010, 499021, 499031, 499043, 499044, 49904X, 499051, 499052, 499060, 499071, 499091, 499094, 499096, 499098, 49909X, 511011, 512011, 512020, 512031, 512041, 512090, 513011, 513020, 513091, 513092, 513093, 514010, 514021, 514022, 514023, 514030, 514041, 514050, 5140XX, 514111, 514120, 514XXX, 515111, 515112, 515113, 516011, 516021, 516031, 516040, 516050, 516063, 516064, 51606X, 516093, 51609X, 517011, 517021, 517041, 517042, 5170XX, 518010, 518021, 518031, 518090, 519010, 519020, 519030, 519041, 519051, 519061, 519071, 519111, 519120, 519151, 519191, 519194, 519195, 519196, 519197, 519198, 5191XX, 531000, 533011, 533020, 533030, 533041, 5330XX, 534010, 534031, 5340XX, 535020, 5350XX, 536021, 536031, 5360XX, 537021, 537030, 537051, 537061, 537062, 537063, 537064, 537070, 537081, 5370XX
Service occupations	211010, 211020, 21109X, 212011, 212021, 212099, 311010, 319011, 319091, 31909X, 331011, 331012, 331021, 331099, 332011, 332020, 333010, 333021, 333050, 3330XX, 339011, 339021, 339030, 339091, 33909X, 351011, 351012, 352010, 352021, 353011, 353021, 353022, 353031, 353041, 359021, 359031, 3590XX, 371011, 371012, 372012, 37201X, 372021, 373010, 391021, 392021, 393010, 393021, 393031, 393090, 394000, 395011, 395012, 395090, 396010, 396030, 397010, 399011, 399021, 399030, 399041, 399099, 536051, 537XXX
Clerical occupations	113071, 131030, 132021, 254031, 411011, 411012, 412010, 412021, 412022, 412031, 413011, 413021, 413031, 413041, 413099, 414010, 419010, 419020, 419031, 419041, 419091, 419099, 431011, 432011, 432021, 432099, 433011, 433021, 433031, 433041, 433051, 433061, 433071, 434011, 434031, 434041, 434051, 434061, 434071, 434081, 434111, 434121, 434131, 434141, 434161, 434171, 434181, 434199, 434XXX, 435011, 435021, 435030, 435041, 435051, 435052, 435053, 435061, 435071, 435081, 435111, 436010, 439011, 439021, 439022, 439041, 439051, 439061, 439071, 439081, 439111, 439XXX

Table A6 – Descriptive statistics of dependent variables

Variable	mean	s.d.	min	median	max
Total employment / pop	0.429	0.066	0.014	0.435	0.956
Employment in abstract occ / pop	0.156	0.042	0.004	0.155	0.327
Employment in manual occ / pop	0.095	0.022	0.003	0.093	0.348
Employment in service occ / pop	0.073	0.012	0.002	0.073	0.154
Employment in clerical occ / pop	0.102	0.018	0.003	0.104	0.173
Green employment / pop	0.020	0.005	0.001	0.020	0.056
Employment in manufacturing / pop	0.041	0.022	0.000	0.038	0.180
Employment in construction / pop	0.020	0.007	0.000	0.019	0.098
Employment in public administration/pop	0.022	0.011	0.000	0.020	0.143
Employment in waste management / pop	0.025	0.009	0.000	0.025	0.108
Average h. wage of manual workers	18.606	3.078	10.167	18.395	102.902
Manual workers with h wage > US-median for manual / pop	0.053	0.013	0.001	0.052	0.238
Manual workers with h wage < US-median for manual / pop	0.042	0.013	0.001	0.041	0.123
Manual workers with > high school degree / pop	0.028	0.007	0.001	0.027	0.135
Manual workers with high school degree or less / pop	0.067	0.017	0.002	0.065	0.213

Notes: data by commuting zone includes only CZ with at least 25000 inhabitants. Statistics weighted by population in 2008.

Appendix B – Quantification of the green ARRA effects

Because we use a log-log model with per capita variables, interpreting the magnitude of our coefficients is challenging. However, converting our elasticities to jobs created per million dollars of ARRA spending produces estimates that are comparable to other papers.

For this conversion, define the predicted value from our model as:

$$\hat{y}_{i,t} = log\left(\frac{Y_{i,t}}{pop_{i,2008}}\right) - log\left(\frac{Y_{i,2008}}{pop_{i,2008}}\right)$$

$$= \alpha + \sum_t \widehat{\beta_t} log\left(\frac{GreenARRA_i}{pop_{i,2008}}\right) + \sum_t \mathbf{X}'_{it_0}\widehat{\boldsymbol{\varphi}}_t + \sum_t \mathbf{G}'_{it_0}\widehat{\boldsymbol{\vartheta}}_t \, , (1)$$

where we skip $\mu_{i\in v,t}$ (vigintiles of non-green ARRA spending) and $\eta_{i\in c,t}$ (period-specific region fixed effects) for simplicity, and t=pre, short and long as usual. We can add \$1 million of green or non-green ARRA and re-calculate:

$$\hat{y}_{i,t}^{+1} = log\left(\frac{Y_{i,t}^{+1}}{pop_{i,2008}}\right) - log\left(\frac{Y_{i,2008}}{pop_{i,2008}}\right)$$

$$= \alpha + \sum_t \widehat{\beta_t} log\left(\frac{GreenARRA_i + 1}{pop_{i,2008}}\right) + \sum_t \mathbf{X}'_{it_0}\widehat{\boldsymbol{\varphi}}_t + \sum_t \mathbf{G}'_{it_0}\widehat{\boldsymbol{\vartheta}}_t \, . (2)$$

Subtracting one from the other gives us:

$$\hat{y}_{i,t}^{+1} - \hat{y}_{i,t} = log\left(\frac{Y_{i,t}^{+1}}{pop_{i,2008}}\right) - log\left(\frac{Y_{i,2008}}{pop_{i,2008}}\right) - log\left(\frac{Y_{i,t}}{pop_{i,2008}}\right) + log\left(\frac{Y_{i,2008}}{pop_{i,2008}}\right)$$

$$= log\left(\frac{Y_{i,t}^{+1}}{pop_{i,2008}}\right) - log\left(\frac{Y_{i,t}}{pop_{i,2008}}\right)$$

$$= \sum_t \widehat{\ } \; log\left(\frac{GreenARRA_i + 1}{pop_{i,2008}}\right) - \sum_t \widehat{\beta_t} \; log\left(\frac{GreenARRA_i}{pop_{i,2008}}\right) . (3)$$

We can re-write the log quotients to simplify further:

$$\hat{y}_{i,t}^{+1} - \hat{y}_{i,t} = log\left(\frac{Y_{i,t}^{+1}}{pop_{i,2008}}\right) - log\left(\frac{Y_{i,t}}{pop_{i,2008}}\right)$$

$$= log(Y_{i,t}^{+1}) - log(pop_{i,2008}) - log(Y_{i,t}) + log(pop_{i,2008})$$

$$= log(Y_{i,t}^{+1}) - log(Y_{i,t}) = log\left(\frac{Y_{i,t}^{+1}}{Y_{i,t}}\right). (4)$$

Converting to levels, we get:

$$exp^{\log\left(\frac{Y_{i,t}^{+1}}{Y_{i,t}}\right)} = \left(\frac{Y_{i,t}^{+1}}{Y_{i,t}}\right). (5)$$

We want

$$Y_{i,t}^{+1} - Y_{i,t} = \left(\frac{Y_{i,t}^{+1}}{Y_{i,t}}\right) Y_{i,t} - Y_{i,t} = Y_{i,t}\left\{exp^{\log\left(\frac{Y_{i,t}^{+1}}{Y_{i,t}}\right)} - 1\right\}.$$

Using (3), (4) and (5) we can replace (Y^{+1}/Y) above with the difference of our predicted values from (3), giving us:

$$Y_{i,t}^{+1} - Y_{i,t} = Y_{i,t}\left\{exp^{\sum_t \widehat{\beta_t}\, log\left(\frac{GreenARRA_i+1}{pop_{i,2008}}\right) - \sum_t \widehat{\beta_t} log\left(\frac{GreenARRA_i}{pop_{i,2008}}\right)} - 1\right\}.$$

For a given time period (e.g. short-run or long-run), this simplifies to:

$$Y_{i,t}^{+1} - Y_{i,t} = Y_{i,t}\left\{exp^{\widehat{\beta_t}\left(log\left(\frac{GreenARRA_i+1}{pop_{i,2008}}\right) - log\left(\frac{GreenARRA_i}{pop_{i,2008}}\right)\right)} - 1\right\}.$$

Appendix C – Robustness Checks

In this Appendix we present a series of robustness checks that address critical aspects of our identification strategy or our definition of green ARRA. For each set of robustness checks, we present results using both state or Census region fixed effects. When our robustness checks change the set of commuting zones included or definition of non-green ARRA, we also recalculate the vigintiles of non-green ARRA. To allow each set of tables to fit on a single page, we omit coefficient estimates and instead present just the calculations for jobs created per $1 million green ARRA.

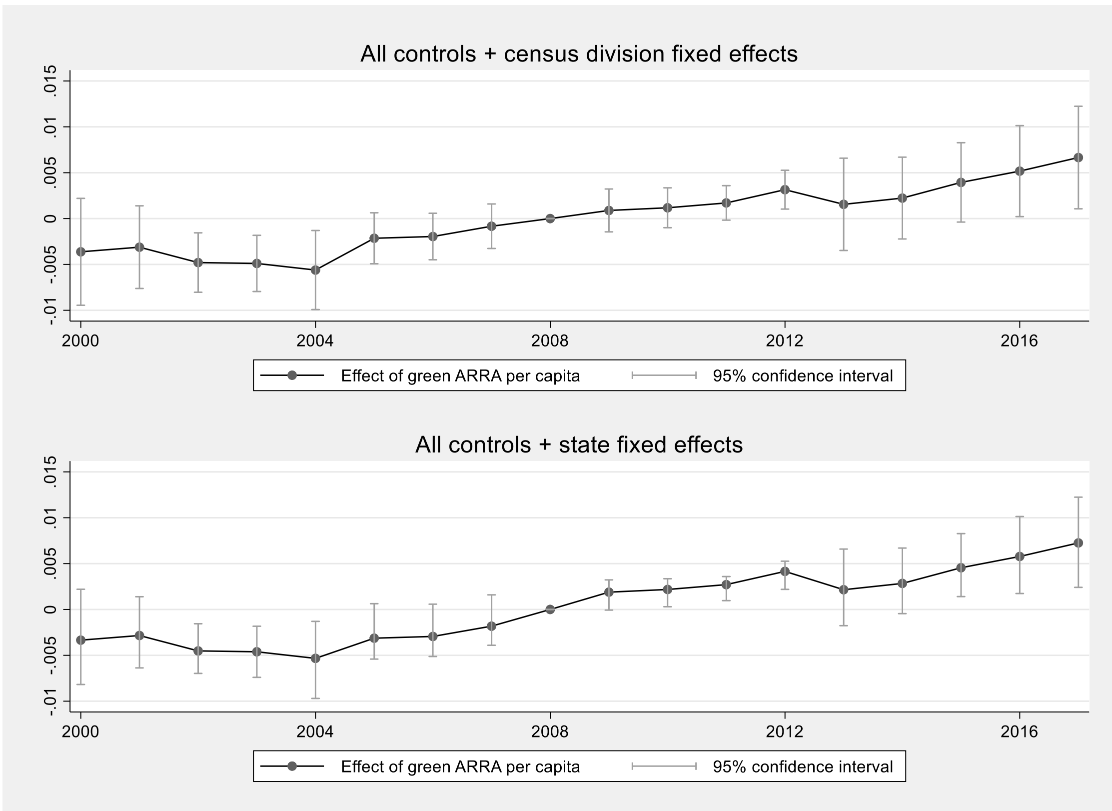
We begin by exploring year-by-year estimates of total employment. Here we allow all the coefficients of equation (1) to vary yearly and use a longer period before 2008 to make the pre and the post periods symmetric covering the period 2000-2017.[23] The visual inspection of the patterns helps interpret our results, as the effect of green ARRA can trend either upwardly or downwardly in the years used to estimate the long-term effect (i.e., 2013 -2017).

We plot the coefficients as well as the 95% confidence intervals for green ARRA in Figure C1. For these regressions only, our dependent variable is $ln\left(\frac{y_{i,t}}{pop_{i,2008}}\right) - ln\left(\frac{y_{i,2008}}{pop_{i,2008}}\right)$ both before and after 2008, so that we can interpret the slope of this plot as the effect of green ARRA on the annualized growth rate in per capita employment between adjacent years.[24] Most notable in this figure is that the pre-trend (green ARRA going to commuting zones with greater employment growth) begins between 2004 and 2005. Prior to that, we observe a flat line, so the estimated pre-

[23] We cannot do this same extension for green or manual employment as in 2001-2004 the American Community Survey data do not report the detailed place of work or place of residence of the respondents.
[24] That is, each coefficient represents the effect of green ARRA on per capita employment relative to the base year of 2008. Thus, the difference between the point estimate in any two adjacent years is the effect of green ARRA on the annual growth rate of employment between those two years.

trend ($\hat{\beta}_{pre}$) in Table 2 overstates the long-term pre-trend using comparable time windows before and the after the Great Recession. In turn, the fact that green ARRA impacts are trending upwards after the crisis indicates that $\hat{\beta}_{long}$ in our main specification is a conservative estimate of the long-term effect. Overall, this analysis reinforces our conclusion that green ARRA spending had a long-term effect on job creation.

Figure C1 – Year-by-year effects



Notes: plot of the annual estimates of log(per capita green ARRA) on the change in log employment per capita compared to 2008 per capita, using the OLS models weighted by CZ population in 2008 (equation 1).

Next, Table C1 shows detailed results of the estimation interacting green skills with green ARRA, presented in Figure 6 in the main text. Of particular note here is that, not only are the interactions statistically significant, but so are the levels of the initial share of occupations in the upper quartile of GGS importance themselves, and this effect is trending upward over time.[25] Recall from Table 1 that the initial share of occupations in the upper quartile of GGS importance is also strongly correlated with the allocation of green ARRA subsidies. In combination, these results reinforce our interpretation of the green stimulus as a successful example of picking the winners.

Tables C2 and C3 consider the importance of particular observations in our data. Column (1) repeats the results from Table 2 in the text. In column (2) we drop observations from 2009. While ARRA spending was announced in 2009, much of the money wasn't allocated until 2010 (Wilson, 2012). Thus, including 2009 in our data may artificially reduce the short-run estimates of job creation. Although we see slightly larger short-run estimates of job creation for total and manual employment when excluding data from 2009, the differences are small. In column (3) we exclude commuting zones in the highest and lowest vigintiles of non-green ARRA spending, as the standard deviation in per capita non-green ARRA is much higher for these two groups, and again observe only small changes in the results. Column (4) excludes commuting zones hosting federal R&D laboratories, which was a key covariate with unbalanced characteristics in Table 1, leading to just slightly larger long-run estimates of green and net manual employment. Finally, in column (5) we show that our results are robust to including small commuting zones (e.g. < 25,000 residents).

Continuing our check of the robustness of our results, Tables C4 and C5 re-run our results using different groupings of non-ARRA spending. In addition to the vigintiles used in the main text (column 4), we consider quintiles of non-green ARRA (column 1), deciles of non-green

[25] A one standard deviation in the green skills share (0.027) accounts, in the most conservative specification with state fixed effects, for a 0.97% difference in employment growth before the crisis that increases up to 1.91% in the short-term and 2.38% in the long-run.

ARRA (column 2) or 15 groups of non-ARRA spending (column 3). Our results are not sensitive to the choice of groupings and the estimates of jobs created are nearly identical in all columns.

Finally, Tables C6 and C7 consider alternative definitions of our ARRA variables. Column (1) repeats the results from Table 2 in the text. In column (2) we add spending on the four Department of Labor training programs mentioned in footnote 7, which provided training for energy efficiency and renewable energy jobs. The four programs are Pathways Out of Poverty, the Energy Training Partnership, Green Capacity Building Grants, and the State Energy Sector Partnership. A total of $496 million was spent on these four programs. We see slightly larger estimates of total and green jobs created (as well as for manual labor when using Census region fixed effects), but also larger pre-trends, so that the net effects are generally similar.

Roughly ten percent of green ARRA supported R&D efforts, primarily for clean energy. One might expect such investments to have little job creation impact. Consistent with that, our estimates of jobs created increase by about 10 percent in the long-run when dropping green R&D from the ARRA data (column 3). However, the short-run results remain similar.

Our ARRA data includes three types of support: grants, contracts, and loans. In column 4 we remove funds for the Department of Energy Loan Guarantee Program. This program supported 23 clean energy projects with loans totaling $12.3 billion – nearly one-quarter of all DOE ARRA investments. Most were for solar or wind (including the controversial loan to Solyndra), although other projects such as energy storage and biomass were also granted loans through this program. Because these loans required payback from the private sector, including such loans could cause our estimates to underestimate the effectiveness of public sector investments. Furthermore, Aldy (2013) argues that these investments were less impactful than other green ARRA investments and took longer to execute. Nearly 2 years after funds were first allocated, the DOE had closed on only

8 of the projects eventually funded. Consistent with these arguments, the effect of green ARRA on employment is slightly larger for manual employment, but not for total or green employment. For total employment higher estimated long-run coefficients are offset by higher pre-trends, which are now significant even when using Census division fixed effects. In column (5) we drop all ARRA loans, including those from other agencies, so that we are comparing similar types of spending across all agencies. Loans were less important for other agencies, with just 2.5 percent of non-green ARRA granted as loans. Thus, not surprisingly, results are similar to omitting the DOE Loan Guarantee program only.

In column (6) we omit contracts from the ARRA data. Just 18 percent of green ARRA and 14 percent of non-green ARRA was awarded as contracts. While many green ARRA contracts were for green services, such as EPA contracts for remediating hazardous waste, some contracts are for administrative work, such as program evaluation and support, that might not be considered green. Removing contracts leads to larger short- and long-run estimates of jobs created for manual labor and larger long-run gains for green employment. Finally, only including ARRA grants (e.g., omitting both loans and contracts, column 7) nearly doubles (or triples with Census division fixed effects) the short-run effect on manual labor and increases the long-run effect by about 50 percent (double with Census division fixed effects). Using only grants has little effect on other employment estimates, although the estimates for green employment become less precise and the pre-trend for total employment is again significant using Census division fixed effects. In total, these robustness checks suggest that including all types of ARRA investments provides a conservative estimate of the potential of properly targeted clean energy subsidies, and that direct grants were more effective at job creation than loans or contracts.

Table C1 – Interaction with initial green skills

Dep var: Change in log employment per capita compared to 2008	State fixed effects	Census division fixed effects
Share of empl with GGS>p75 (year 2005) x D2005_2007	0.3633*	0.4763**
	(0.1988)	(0.2265)
Share of empl with GGS>p75 (year 2005) x D2009_2012	0.6999**	1.1190***
	(0.3001)	(0.3093)
Share of empl with GGS>p75 (year 2005) x D2013_2017	0.8717*	1.4937***
	(0.4930)	(0.5263)
Green ARRA per capita (log) x D2005_2007	-0.0054	-0.0091*
	(0.0048)	(0.0054)
Green ARRA per capita (log) x D2009_2012	-0.0149*	-0.0248***
	(0.0075)	(0.0078)
Green ARRA per capita (log) x D2013_2017	-0.0225*	-0.0376***
	(0.0125)	(0.0135)
Green ARRA per capita (log) x Share of empl with GGS>p75 (year 2005) x D2005_2007	0.0323	0.0438*
	(0.0199)	(0.0221)
Green ARRA per capita (log) x Share of empl with GGS>p75 (year 2005) x D2009_2012	0.0709**	0.1081***
	(0.0304)	(0.0310)
Green ARRA per capita (log) x Share of empl with GGS>p75 (year 2005) x D2013_2017	0.1097**	0.1689***
	(0.0485)	(0.0507)
Jobs created, $1 million green ARRA:		
- First quartile of Share of empl with GGS>p75 in 2006 (0.235)		
Pre-ARRA (2005-2007)	9.98***	5.34
	(3.64)	(4.99)
Short-run - pre-ARRA	-1.75	-2.5
	(3.23)	(3.83)
Long-run - pre-ARRA	5.22	4.62
	(7.81)	(9.93)
- Median of Share of empl with GGS>p75 in 2006 (0.251)		
Pre-ARRA (2005-2007)	12.25***	8.43
	(3.87)	(5.16)
Short-run - pre-ARRA	0.87	1.86
	(3.52)	(3.96)
Long-run - pre-ARRA	10.84	13.71
	(7.73)	(9.09)
- Third quartile of Share of empl with GGS>p75 in 2006 (0.269)		
Pre-ARRA (2005-2007)	14.51***	11.5*
	(4.54)	(5.76)
Short-run - pre-ARRA	3.48	6.2
	(4.58)	(4.73)
Long-run - pre-ARRA	16.43*	22.75**
	(9.05)	(9.50)
R squared	0.7688	0.6858
Observations	7631	7631

Notes: OLS model weighted by CZ population in 2008. Sample: 587 CZ with at least 25,000 residents in 2008. Year fixed effects and state (or Census region) x period fixed effects included. Additional control variables (interacted with D2005_2007, D2009_2012 and D2013_2017 dummies) same as Table 2. Standard errors clustered by state in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C2 – Robustness checks: excluding or including observations (state fixed effects)

Dep var: Change in log employment per capita compared to 2008	(1) Main Model	(2) Drop 2009	(3) Excluding 1st and 20th vigintiles	(4) Excluding CZs hosting Federal R&D Labs	(5) Including CZs with less than 25k residents
Total Employment					
Jobs created, $1 million green ARRA:					
Pre-ARRA (2005-2007)	11.53***	11.53***	7.16*	12.06**	11.26***
	(3.85)	(3.85)	(3.74)	(4.75)	(3.57)
Short-run (2009-2012)	11.15***	12.18***	10.69**	9.91***	9.51***
	(3.29)	(3.78)	(4.50)	(3.46)	(3.11)
Long-run (2013-2017)	20.8***	20.8***	19.85**	20.92**	20.88***
	(7.37)	(7.38)	(9.52)	(8.05)	(6.06)
Short-run - pre-ARRA	0.03	1.06	3.78	-1.72	-1.34
	(3.49)	(4.10)	(4.62)	(3.55)	(2.88)
Long-run - pre-ARRA	8.92	8.92	12.46	8.48	9.28
	(8.02)	(8.03)	(9.57)	(7.78)	(6.59)
R squared	0.7672	0.7571	0.7875	0.7218	0.7440
Green Employment					
Jobs created, $1 million green ARRA:					
Pre-ARRA (2005-2007)	0	0	0.54	-0.13	0.09
	(0.87)	(0.87)	(1.20)	(0.75)	(0.85)
Short-run (2009-2012)	0.78	1.23	0.32	0.77	0.91
	(0.76)	(0.86)	(0.92)	(0.78)	(0.74)
Long-run (2013-2017)	2.66**	2.66**	1.59	3.11***	2.81**
	(1.11)	(1.11)	(1.48)	(1.13)	(1.10)
Short-run - pre-ARRA	0.78	1.23	-0.2	0.9	0.82
	(1.49)	(1.58)	(1.94)	(1.40)	(1.48)
Long-run - pre-ARRA	2.66	2.66	1	3.26*	2.71
	(1.83)	(1.83)	(2.36)	(1.75)	(1.80)
R squared	0.4159	0.4140	0.4268	0.3561	0.4117
Manual Labor Employment					
Jobs created, $1 million green ARRA:					
Pre-ARRA (2005-2007)	0.92	0.92	-3.38	-1.24	0.44
	(2.98)	(2.98)	(2.76)	(4.05)	(2.61)
Short-run (2009-2012)	5.48**	7.38***	6.14*	6.17***	4.33**
	(2.10)	(2.38)	(3.09)	(2.20)	(2.15)
Long-run (2013-2017)	11.34**	11.34**	11.94	11.26**	9.32**
	(4.80)	(4.81)	(7.38)	(4.69)	(4.24)
Short-run - pre-ARRA	4.7	6.59*	9.05*	7.24	3.95
	(3.39)	(3.44)	(4.76)	(4.61)	(2.95)
Long-run - pre-ARRA	10.48*	10.48*	15.11*	12.43**	8.91*
	(5.46)	(5.47)	(8.38)	(5.31)	(4.64)
R squared	0.5749	0.5774	0.6006	0.5461	0.5554
Observations	7631	7044	6864	7319	8957

Notes: OLS model weighted by CZ population in 2008. Sample: CZ with at least 25,000 residents in 2008 (except column 5). Year fixed effects and state x period fixed effects included. Additional control variables (interacted with D2005_2007, D2009_2012 and D2013_2017 dummies) same as Table 2, except that vigintiles of non-green ARRA spending are re-calculated in columns (4) and (5) to reflect the new set of observations. Standard errors clustered by state in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C3 – Robustness checks: excluding or including observations (census division F.E.)

Dep var: Change in log employment per capita compared to 2008	(1) Main Model	(2) Drop 2009	(3) Excluding 1st and 20th vigintiles	(4) Excluding CZs hosting Federal R&D Labs	(5) Including CZs with less than 25k residents
Total Employment					
Jobs created, $1 million green ARRA:					
Pre-ARRA (2005-2007)	7.35	7.35	1.63	6.68	7.52*
	(4.94)	(4.94)	(5.51)	(5.45)	(4.45)
Short-run (2009-2012)	7.42*	8.62*	3.51	6.73	8.09**
	(3.95)	(4.48)	(4.79)	(4.21)	(3.49)
Long-run (2013-2017)	18.03*	18.03*	11.23	18.93*	20.93***
	(10.15)	(10.16)	(11.77)	(10.57)	(7.37)
Short-run - pre-ARRA	0.33	1.53	1.95	0.3	0.84
	(4.05)	(4.70)	(5.73)	(4.33)	(3.71)
Long-run - pre-ARRA	10.45	10.45	9.55	12.04	13.18*
	(9.46)	(9.47)	(11.29)	(9.81)	(7.21)
R squared	0.6819	0.6649	0.7013	0.6357	0.6539
Green Employment					
Jobs created, $1 million green ARRA:					
Pre-ARRA (2005-2007)	-0.07	-0.07	0.48	-0.73	-0.23
	(0.85)	(0.86)	(1.11)	(0.81)	(0.84)
Short-run (2009-2012)	-0.3	0.05	-1.28	0.16	0.11
	(0.92)	(1.06)	(0.95)	(0.84)	(0.84)
Long-run (2013-2017)	1.84	1.84	0.31	2.66*	2.2*
	(1.34)	(1.34)	(1.55)	(1.33)	(1.25)
Short-run - pre-ARRA	-0.24	0.11	-1.74	0.87	0.33
	(1.58)	(1.69)	(1.79)	(1.42)	(1.51)
Long-run - pre-ARRA	1.92	1.92	-0.23	3.47*	2.46
	(1.97)	(1.97)	(2.27)	(1.91)	(1.93)
R squared	0.3336	0.3267	0.3483	0.2687	0.3311
Manual Labor Employment					
Jobs created, $1 million green ARRA:					
Pre-ARRA (2005-2007)	-0.47	-0.47	-3.95	-3.3	-2.06
	(3.10)	(3.10)	(3.44)	(4.13)	(3.05)
Short-run (2009-2012)	3.2	4.91	1.73	4.93**	3.65
	(2.77)	(3.17)	(3.94)	(2.39)	(2.49)
Long-run (2013-2017)	10.76	10.76	9.43	11.32*	10.76**
	(6.46)	(6.46)	(8.57)	(6.11)	(5.35)
Short-run - pre-ARRA	3.61	5.31	5.13	7.77*	5.43
	(3.84)	(4.01)	(5.89)	(4.16)	(3.55)
Long-run - pre-ARRA	11.2*	11.2*	13.13	14.41**	12.7**
	(6.46)	(6.46)	(9.59)	(5.93)	(5.62)
R squared	0.4907	0.4858	0.5105	0.4677	0.4740
Observations	7631	7044	6864	7319	8957

Notes: OLS model weighted by CZ population in 2008. Sample: CZ with at least 25,000 residents in 2008 (except column 5). Year fixed effects and census division x period fixed effects included. Additional control variables (interacted with D2005_2007, D2009_2012 and D2013_2017 dummies) same as Table 2, except that vigintiles of non-green ARRA spending are re-calculated in columns (4) and (5) to reflect the new set of observations. Standard errors clustered by state in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C4 – Robustness checks: Alternate groupings of non-green ARRA (state fixed effects)

Dep var: Change in log employment per capita compared to 2008	(1) 5 non-green ARRA groups	(2) 10 non-green ARRA groups	(3) 15 non-green ARRA groups	(4) 20 non-green ARRA groups
Total Employment				
Jobs created, $1 million green ARRA:				
Pre-ARRA (2005-2007)	11.63***	11.22***	12.55***	11.53***
	(3.38)	(3.44)	(3.35)	(3.85)
Short-run (2009-2012)	10.18***	10.49***	11.99***	11.15***
	(3.53)	(3.27)	(3.49)	(3.29)
Long-run (2013-2017)	18.42**	20.22***	25.29***	20.8***
	(7.47)	(7.07)	(7.78)	(7.37)
Short-run - pre-ARRA	-1.03	-0.33	-0.11	0.03
	(3.78)	(3.59)	(3.48)	(3.49)
Long-run - pre-ARRA	6.44	8.66	12.35	8.92
	(8.20)	(7.84)	(8.09)	(8.02)
R squared	0.7562	0.7585	0.7622	0.7672
Green Employment				
Jobs created, $1 million green ARRA:				
Pre-ARRA (2005-2007)	0.31	-0.01	0.2	0
	(0.96)	(0.92)	(0.94)	(0.87)
Short-run (2009-2012)	0.51	0.86	0.69	0.78
	(0.79)	(0.75)	(0.80)	(0.76)
Long-run (2013-2017)	2.23**	2.62**	2.73**	2.66**
	(1.10)	(1.18)	(1.14)	(1.11)
Short-run - pre-ARRA	0.22	0.87	0.5	0.78
	(1.62)	(1.54)	(1.60)	(1.49)
Long-run - pre-ARRA	1.89	2.63	2.51	2.66
	(1.92)	(1.94)	(1.94)	(1.83)
R squared	0.4023	0.4096	0.4111	0.4159
Manual Labor Employment				
Jobs created, $1 million green ARRA:				
Pre-ARRA (2005-2007)	1.79	1.21	1.68	0.92
	(2.49)	(2.69)	(2.98)	(2.98)
Short-run (2009-2012)	5.24**	5.36***	4.94**	5.48**
	(2.08)	(1.91)	(2.12)	(2.10)
Long-run (2013-2017)	11.17**	11**	11.15**	11.34**
	(4.33)	(4.33)	(4.50)	(4.80)
Short-run - pre-ARRA	3.7	4.32	3.5	4.7
	(2.91)	(2.81)	(3.39)	(3.39)
Long-run - pre-ARRA	9.5*	9.87**	9.58*	10.48*
	(4.79)	(4.77)	(5.05)	(5.46)
R squared	0.5591	0.5620	0.5677	0.5749
Observations	7631	7631	7631	7631

Notes: OLS model weighted by CZ population in 2008. Sample: CZ with at least 25,000 residents in 2008. Year fixed effects and state x period fixed effects included. Additional control variables (interacted with D2005_2007, D2009_2012 and D2013_2017 dummies) same as Table 2. Standard errors clustered by state in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C5 – Robustness checks: Alternate groupings of non-green ARRA (census division F.E.)

Dep var: Change in log employment per capita compared to 2008	(1) 5 non-green ARRA groups	(2) 10 non-green ARRA groups	(3) 15 non-green ARRA groups	(4) 20 non-green ARRA groups
Total Employment				
Jobs created, $1 million green ARRA:				
Pre-ARRA (2005-2007)	7.95*	6.74	7.4	7.35
	(4.60)	(4.85)	(4.67)	(4.94)
Short-run (2009-2012)	7.85**	7.32*	8.61**	7.42*
	(3.89)	(4.03)	(3.92)	(3.95)
Long-run (2013-2017)	16.2*	16.55*	21.52**	18.03*
	(9.26)	(9.76)	(10.32)	(10.15)
Short-run - pre-ARRA	0.18	0.82	1.48	0.33
	(4.44)	(4.28)	(3.92)	(4.05)
Long-run - pre-ARRA	8.01	9.6	13.9	10.45
	(9.36)	(9.30)	(9.42)	(9.46)
R squared	0.6622	0.6688	0.6741	0.6819
Green Employment				
Jobs created, $1 million green ARRA:				
Pre-ARRA (2005-2007)	0.1	-0.07	-0.06	-0.07
	(0.94)	(0.90)	(0.94)	(0.85)
Short-run (2009-2012)	-0.21	-0.22	-0.23	-0.3
	(0.86)	(0.89)	(0.91)	(0.92)
Long-run (2013-2017)	1.58	1.62	2.03	1.84
	(1.19)	(1.36)	(1.33)	(1.34)
Short-run - pre-ARRA	-0.31	-0.16	-0.17	-0.24
	(1.62)	(1.59)	(1.67)	(1.58)
Long-run - pre-ARRA	1.47	1.69	2.09	1.92
	(1.92)	(2.02)	(2.07)	(1.97)
R squared	0.3189	0.3251	0.3333	0.3336
Manual Labor Employment				
Jobs created, $1 million green ARRA:				
Pre-ARRA (2005-2007)	0.3	-0.34	-0.17	-0.47
	(2.71)	(2.99)	(3.20)	(3.10)
Short-run (2009-2012)	3.94	3.55	3.48	3.2
	(2.61)	(2.63)	(2.59)	(2.77)
Long-run (2013-2017)	11.55*	10.65*	11.66*	10.76
	(6.00)	(6.02)	(6.18)	(6.46)
Short-run - pre-ARRA	3.68	3.84	3.62	3.61
	(3.35)	(3.45)	(3.80)	(3.84)
Long-run - pre-ARRA	11.28*	10.96*	11.81*	11.2*
	(6.07)	(6.00)	(6.27)	(6.46)
R squared	0.4686	0.4731	0.4861	0.4907
Observations	7631	7631	7631	7631

Notes: OLS model weighted by CZ population in 2008. Sample: CZ with at least 25,000 residents in 2008. Year fixed effects and census division x period fixed effects included. Additional control variables (interacted with D2005_2007, D2009_2012 and D2013_2017 dummies) same as Table 2. Standard errors clustered by state in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C6 – Robustness checks: Alternative ARRA definitions (state fixed effects)

Dep var: Change in log employment per capita compared to 2008	(1) Main Model	(2) Include DOL training	(3) Exclude energy R&D	(4) Drop DOE Loans	(5) Drop All Loans	(6) Drop Contracts	(7) Grants Only
Total Employment							
Jobs created, $1 million green ARRA:							
Pre-ARRA (2005-2007)	11.53***	12.8***	14.17***	15.53***	17.19***	16.39***	18.97***
	(3.85)	(3.85)	(4.54)	(4.24)	(4.19)	(4.16)	(5.33)
Short-run (2009-2012)	11.15***	11.27***	11.92***	13.51***	13.45***	11.19**	13.67***
	(3.29)	(3.30)	(3.66)	(4.10)	(4.05)	(4.44)	(5.08)
Long-run (2013-2017)	20.8***	21.74**	22.73***	20.82**	21.76**	23.94**	25.18**
	(7.37)	(8.41)	(7.80)	(9.47)	(10.09)	(9.35)	(10.76)
Short-run - pre-ARRA	0.03	-1.08	-1.75	-1.42	-3.08	-4.62	-4.6
	(3.49)	(3.23)	(3.78)	(3.76)	(3.20)	(4.18)	(4.49)
Long-run - pre-ARRA	8.92	8.57	8.11	4.85	4.08	7.07	5.63
	(8.02)	(8.63)	(8.47)	(9.37)	(9.38)	(9.68)	(10.71)
R squared	0.7672	0.7696	0.7672	0.7667	0.7691	0.7676	0.7653
Green Employment							
Jobs created, $1 million green ARRA:							
Pre-ARRA (2005-2007)	0	0.22	0.38	0.31	0.42	0.38	0.44
	(0.87)	(0.90)	(0.97)	(1.05)	(1.05)	(0.93)	(1.11)
Short-run (2009-2012)	0.78	0.85	0.71	0.82	0.94	0.69	0.82
	(0.76)	(0.78)	(0.84)	(0.95)	(0.95)	(1.01)	(1.23)
Long-run (2013-2017)	2.66**	2.71**	2.95**	2.43*	2.52*	2.74*	2.74
	(1.11)	(1.22)	(1.21)	(1.45)	(1.48)	(1.40)	(1.79)
Short-run - pre-ARRA	0.78	0.64	0.34	0.52	0.54	0.32	0.4
	(1.49)	(1.56)	(1.67)	(1.86)	(1.85)	(1.82)	(2.21)
Long-run - pre-ARRA	2.66	2.47	2.53	2.09	2.07	2.32	2.26
	(1.83)	(1.93)	(2.04)	(2.38)	(2.38)	(2.25)	(2.86)
R squared	0.4159	0.4151	0.4159	0.4151	0.4143	0.4219	0.4177
Manual Labor Employment							
Jobs created, $1 million green ARRA:							
Pre-ARRA (2005-2007)	0.92	1.47	2.26	0.79	1.32	1.57	-0.37
	(2.98)	(2.41)	(3.37)	(3.82)	(3.05)	(3.70)	(4.66)
Short-run (2009-2012)	5.48**	4.6*	5.4**	7.03***	6.39**	7.26***	9.28***
	(2.10)	(2.30)	(2.25)	(2.49)	(2.48)	(2.38)	(2.45)
Long-run (2013-2017)	11.34**	10.25**	12.25**	14.27**	13.06**	13.13***	16.08***
	(4.80)	(4.58)	(4.89)	(6.03)	(5.57)	(4.15)	(5.35)
Short-run - pre-ARRA	4.7	3.34	3.45	6.35	5.26	5.91	9.59*
	(3.39)	(2.94)	(3.91)	(4.30)	(3.53)	(4.03)	(4.94)
Long-run - pre-ARRA	10.48*	8.88*	10.13*	13.53*	11.83**	11.66**	16.42**
	(5.46)	(4.67)	(5.90)	(6.79)	(5.79)	(4.85)	(6.47)
R squared	0.5749	0.5647	0.5748	0.5752	0.5652	0.5749	0.5730
Observations	7631	7631	7631	7631	7631	7631	7631

Notes: OLS model weighted by CZ population in 2008. Sample: CZ with at least 25,000 residents in 2008. Year fixed effects and state x period fixed effects included. Additional control variables (interacted with D2005_2007, D2009_2012 and D2013_2017 dummies) same as Table 2, except that vigintiles of non-green ARRA spending are re-calculated in columns (2) and (5)-(7) to reflect the new definition of non-green ARRA. Standard errors clustered by state in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C7 – Robustness checks: Alternative ARRA definitions (census division fixed effects)

Dep var: Change in log employment per capita compared to 2008	(1) Main Model	(2) Include DOL training	(3) Exclude energy R&D	(4) Drop DOE Loans	(5) Drop All Loans	(6) Drop Contracts	(7) Grants Only
Total Employment							
Jobs created, $1 million green ARRA:							
Pre-ARRA (2005-2007)	7.35	9.7**	7	16.03***	18.01***	6.74	13.44**
	(4.94)	(4.80)	(5.67)	(4.30)	(4.28)	(6.24)	(5.66)
Short-run (2009-2012)	7.42*	9.57**	7.94*	12.42**	13.94***	7.01	12.73**
	(3.95)	(3.81)	(4.46)	(4.87)	(4.87)	(5.00)	(5.91)
Long-run (2013-2017)	18.03*	21.96**	18.97*	30.04**	32.85**	16.99	33.31**
	(10.15)	(10.17)	(11.23)	(12.36)	(12.97)	(11.87)	(14.45)
Short-run - pre-ARRA	0.33	0.22	1.18	-2.99	-3.38	0.5	-0.21
	(4.05)	(4.03)	(4.72)	(4.36)	(4.02)	(5.29)	(6.25)
Long-run - pre-ARRA	10.45	11.99	11.75	13.55	14.33	10.05	19.47
	(9.46)	(9.29)	(10.18)	(11.43)	(11.48)	(10.66)	(14.77)
R squared	0.6819	0.6926	0.6817	0.6837	0.6945	0.6833	0.6818
Green Employment							
Jobs created, $1 million green ARRA:							
Pre-ARRA (2005-2007)	-0.07	-0.22	0.15	0.57	0.38	-0.44	-0.04
	(0.85)	(0.89)	(0.93)	(1.06)	(1.04)	(0.92)	(1.12)
Short-run (2009-2012)	-0.3	0.24	-0.43	0.04	0.46	-0.24	0.24
	(0.92)	(0.88)	(0.96)	(1.09)	(1.05)	(1.10)	(1.36)
Long-run (2013-2017)	1.84	2.52*	1.9	2.75*	3.12*	1.17	2.77
	(1.34)	(1.43)	(1.47)	(1.56)	(1.68)	(1.78)	(2.08)
Short-run - pre-ARRA	-0.24	0.46	-0.58	-0.5	0.09	0.17	0.28
	(1.58)	(1.61)	(1.69)	(1.98)	(1.92)	(1.80)	(2.30)
Long-run - pre-ARRA	1.92	2.76	1.73	2.12	2.7	1.65	2.81
	(1.97)	(2.14)	(2.15)	(2.47)	(2.56)	(2.46)	(3.15)
R squared	0.3336	0.3402	0.3335	0.3341	0.3404	0.3417	0.3415
Manual Labor Employment							
Jobs created, $1 million green ARRA:							
Pre-ARRA (2005-2007)	-0.47	-0.5	0.37	0.38	0.64	-1.69	-3.73
	(3.10)	(3.10)	(3.39)	(3.72)	(3.48)	(3.82)	(4.91)
Short-run (2009-2012)	3.2	4.05	2.44	6.95**	7.51***	5.24*	9.72***
	(2.77)	(2.62)	(2.95)	(2.88)	(2.49)	(2.87)	(2.58)
Long-run (2013-2017)	10.76	11.99*	11.32	18.62**	19.45***	14.65**	22.84***
	(6.46)	(5.97)	(6.85)	(7.22)	(6.59)	(6.42)	(6.50)
Short-run - pre-ARRA	3.61	4.48	2.12	6.63	6.97*	6.7*	12.92**
	(3.84)	(3.74)	(4.34)	(4.48)	(4.00)	(3.95)	(5.24)
Long-run - pre-ARRA	11.2*	12.46**	10.98	18.27**	18.85***	16.24***	26.31***
	(6.46)	(5.78)	(6.92)	(7.70)	(6.90)	(5.98)	(7.54)
R squared	0.4907	0.4852	0.4905	0.4934	0.4881	0.4868	0.4879
Observations	7631	7631	7631	7631	7631	7631	7631

Notes: OLS model weighted by CZ population in 2008. Sample: CZ with at least 25,000 residents in 2008. Year fixed effects and census division x period fixed effects included. Additional control variables (interacted with D2005_2007, D2009_2012 and D2013_2017 dummies) same as Table 2, except that vigintiles of non-green ARRA spending are re-calculated in columns (2) and (5)-(7) to reflect the new definition of non-green ARRA. Standard errors clustered by state in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Appendix D – Instrumental variable results

As noted in the main text, our instrumental variable results use a shift-share instrument that combines the initial "share" of EPA plus DOE spending in the CZ (over total DOE and EPA spending) with the green ARRA "shift". Such instrument adds an exogenous shock in green expenditures to areas that were already receiving larger amount of green spending before ARRA. The instrument is formally defined as:

$$IV_i = \frac{DoE\ Pre{-}ARRA_{i,2003-04}}{DoE\ Pre{-}ARRA_{2003-04}} \times \frac{Green\ ARRA\ DoE}{Pop_{2008}} + \frac{EPA\ Pre{-}ARRA_{i,2003-04}}{EPA\ Pre{-}ARRA_{2003-04}} \times \frac{Green\ ARRA\ EPA}{Pop_{2008}},$$

where total green ARRA EPA and DOE per capita is reallocated to CZs depending on their respective pre-ARRA shares of spending over the national total, i.e. $\frac{DoE\ Pre{-}ARRA_{i,2003-04}}{DoE\ Pre{-}ARRA_{2003-04}}$ and $\frac{EPA\ Pre{-}ARRA_{i,2003-04}}{EPA\ Pre{-}ARRA_{2003-04}}$.

Because such an instrument adds an exogenous shock in green expenditures to areas that were already receiving larger green investments before ARRA, we face a problem similar to that put forward by Jaeger et al. (2018), who note that a shift-share instrument conflates short- and long-term effects. We follow their suggestion and take a "share" far in the past (i.e. an average share of DOE plus EPA spending between 2003 and 2004), under the assumption that the effect of past spending gradually fades away and thus it is excludable from the second stage.

Unfortunately, developing a reliable measure of pre-ARRA green government spending to distinguish the additional contribution of green ARRA from that of past trends associated with pre-ARRA green spending is difficult with available data. Quality data on green spending before ARRA would enable us to clearly disentangle the effect of ARRA from that of past government spending. Data on local government spending are publicly available at USASPENDING.GOV. However, for two reasons these data are not good proxies of local green spending before ARRA.

First, while EPA spending could be considered as 'green' both during ARRA and prior of ARRA, the same is not true for DOE. While a very large part of DOE local spending in ARRA goes to fund renewable energy investments, energy efficiency and other green programmes (Aldy, 2013), much DOE spending in earlier years was aimed at the exploitation and use of fossil fuels and nuclear energy (Department of Energy Budget Highlights, various years). More importantly, local spending for assistance available at USASPENDING.gov (e.g. CFDA Catalogue of Federal Domestic Assistance) is attributed to the prime recipient while sub-awards are consistently recorded only starting from 2010-2012 onwards. As a result, assistance given to local state governments to be distributed to countries is recorded as fully attributed to the CZ where the state capital is. Despite these important limitations, we do observe a relatively strong correlation (0.485) between DOE+EPA local spending per capita in 2005-2007 and DOE+EPA (i.e. green) ARRA spending per capita. Overall, we can use these data to build our instrument but not as a direct proxy of pre-ARRA spending.

For our shift-share instrument, we use all assistance from the DOE and EPA in 2003 and 2004. While our ARRA data include contracts, we do not include contracts in our instrument. Contracts make up the majority of 2003-2004 spending in USASpending.gov. 82% of DOE & EPA spending is from contracts, and just 18% from assistance. However, many of these contracts are for providing basic services, such as IT services. In contrast, there are fewer contracts in the ARRA data – just 18 percent of green ARRA were from contracts. These are generally contracts that are relevant for green jobs, such as hazardous waste remediation. Thus, while contracts are appropriate to include in our green ARRA data, the contracts in USASpending.gov are not comparable. Our robustness analysis in Appendix C shows that our main results are robust to excluding contracts from the ARRA data.

Finally, since not all DOE spending is green, we created an alternative instrument that only included "green" spending from the DOE, which we identified using CFDA titles. These programs represented 37% of DOE spending in 2003-04. However, limiting the instrument to only green DOE spending did not improve the fit of the instrument and raises potential endogeneity concerns. Thus, we include all DOE spending in our shift-share instrument.

Table D1 presents the first-stage estimation using our shift-share instrument. The instrument does have a statistically significant positive impact on per-capita green ARRA investments. However, the F-stat of the instrument only exceeds 10 when using Census division fixed effects. The weak instrument problem is consistent with green ARRA redirecting DOE spending towards green programs.

Table D1 – First stage IV

Dep var: Green (EPA+DoE) ARRA per capita (in log)	State fixed effects	Census division fixed effects
Shift-share IV for green ARRA	0.0497***	0.0509***
	(0.0181)	(0.0159)
R squared	0.4494	0.3996
F-test of excluded IV from first stage	7.52	10.21
N	587	587

Notes: OLS model weighted by CZ population in 2008. Sample: CZ with at least 25,000 residents in 2008. Standard errors clustered by state in parentheses. * p<0.1, ** p<0.05, *** p<0.01. , Control variables: Vigintiles of non-green ARRA per capita Share of empl with GGS>p75 (year 2006), Population 2008 (log), Income per capita (2005), Import penetration (year 2005), Pre trend (2000-2007) empl manufacturing / pop, Pre trend (2000-2007) employment tot / pop, Pre trend (2000-2007) empl constr / pop, Pre trend (2000-2007) empl extractive / pop, Pre trend (2000-2007) empl public sect / pop, Pre trend (2000-2007) unempl / pop, Pre trend (2000-2007) empl edu health / pop, Empl manuf (average 2006-2008) / pop, Empl constr (average 2006-2008) / pop, Empl extractive (average 2006-2008) / pop, Empl public sect (average 2006-2008) / pop, Unempl (average 2006-2008) / pop, Empl edu health (average 2006-2008) / pop, Shale gas extraction in CZ interacted with year dummies, Potential for wind energy interacted with year dummies, Potential for photovoltaic energy interacted with year dummies, Federal R&D lab, CZ hosts the state capital, Nonattainment CAA old standards, Nonattainment CAA new standards.

Table D2 shows our instrumental variable results. As noted in the main text, the IV estimation overstates both the pre-trends for total employment ($\hat{\beta}_{pre}$), increasing the pre-trend in each regression by an order of magnitude compared to the OLS results. We also observe larger total and net effects of green ARRA on employment. As expected, these effects are imprecisely estimated due to the weak instrument problem. Although the IV results are still informative, suggesting that the effect of green ARRA is highly heterogeneous and much stronger on compliers, they exacerbate the source of endogeneity associated with the presence of pre-trends. Thus, we focus on the OLS results in the main text of the paper.

Table D2 – Instrumental variable results

	IV, state fixed effects			IV, census division fixed effects		
Dep var: Change in log employment (by type) per capita compared to 2008	Total employment	Green employment	Manual occupations	Total employment	Green employment	Manual occupations
Green ARRA per capita (log) x D2005_2007	0.0142**	-0.0093	0.0064	0.0108*	-0.0008	0.0047
	(0.0056)	(0.0241)	(0.0200)	(0.0057)	(0.0219)	(0.0193)
Green ARRA per capita (log) x D2009_2012	0.0167***	0.0306	0.0138	0.0122**	0.0076	0.0059
	(0.0059)	(0.0316)	(0.0162)	(0.0056)	(0.0287)	(0.0135)
Green ARRA per capita (log) x D2013_2017	0.0355***	0.0725**	0.0362*	0.0281**	0.0376	0.0216
	(0.0117)	(0.0350)	(0.0205)	(0.0114)	(0.0340)	(0.0187)
Jobs created, $1 million green ARRA:						
Pre-ARRA (2005-2007)	63.47**	-1.87	7.23	48.34*	-0.16	5.24
	(25.18)	(4.86)	(22.51)	(25.49)	(4.41)	(21.70)
Short-run (2009-2012)	72.05***	5.94	13.28	52.73**	1.47	5.69
	(25.44)	(6.14)	(15.69)	(24.40)	(5.56)	(13.07)
Long-run (2013-2017)	163.95***	16.2**	38.09*	129.74**	8.38	22.73
	(54.37)	(7.86)	(21.68)	(52.53)	(7.59)	(19.72)
Short-run - pre-ARRA	10.85	7.73	7.07	6.12	1.62	1.18
	(18.23)	(10.20)	(30.93)	(19.84)	(8.89)	(26.15)
Long-run - pre-ARRA	98.53**	18.25	31.33	79.91*	8.55	17.82
	(45.10)	(12.41)	(36.33)	(44.38)	(11.60)	(30.96)
R squared	0.5487	0.3061	0.5242	0.5004	0.2656	0.4512
Observations	7631	7631	7631	7631	7631	7631
F-stat of excluded instruments for IV	7.52	7.52	7.52	10.21	10.21	10.21

Notes: Regressions weighted by CZ population in 2008. Sample: 587 CZ with at least 25,000 residents in 2008. Year fixed effects and state (or census division) x period fixed effects included. Additional control variables (interacted with D2005_2007, D2009_2012 and D2013_2017 dummies): Vigintiles of non-green ARRA per capita, Share of empl with GGS>p75 (2005), Population 2008 (log), Income per capita (2005), Import penetration (year 2005), Pre trend (2000-2007) empl manufacturing / pop, Pre trend (2000-2007) employment tot / pop, Pre trend (2000-2007) empl constr / pop, Pre trend (2000-2007) empl extractive / pop, Pre trend (2000-2007) empl public sect / pop, Pre trend (2000-2007) unempl / pop, Pre trend (2000-2007) empl edu health / pop, Empl manuf (average 2006-2008) / pop, Empl constr (average 2006-2008) / pop, Empl extractive (average 2006-2008) / pop, Empl public sect (average 2006-2008) / pop, Unempl (average 2006-2008) / pop, Empl edu health (average 2006-2008) / pop, Shale gas extraction in CZ interacted with year dummies, Potential for wind energy interacted with year dummies, Potential for photovoltaic energy interacted with year dummies, Federal R&D lab, CZ hosts the state capital, Nonattainment CAA old standards, Nonattainment CAA new standards. Endogenous variable (columns 3 and 4): Green ARRA per capita (log). Excluded IV from the first stage: shift-share IV of ARRA spending by Department/Agency; local spending share 2001-2004. Standard errors clustered by state in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

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