Impact evaluation of labour market and social policies through the use of linked administrative and survey data

Technical report: Impact Evaluation of Training and Wage Subsidies for the Unemployed in Greece

February 2024

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1 Executive summary

This report is part of a project between the OECD and the European Commission (EC) that aims to facilitate the use of high-quality data to evaluate the outcomes and effectiveness of labour market policies (ALMPs).[1] The broader aim of the project is to improve the capacity of countries to better evaluate and design policies to benefit their citizens. As part of the OECD-EC project, the OECD conducted a counterfactual impact evaluation (CIE) of Greece's training and wage subsidy programmes implemented during the 2017-21 period. The results of the evaluations have been published in the OECD publication series Connecting People with Jobs (OECD, 2024[1]). This technical report accompanies the report on the results of the evaluation and provides more detail on the data and techniques underlying the main report, as well as more context on some of their main strengths and weaknesses. It includes a discussion of the different data sources available for analysis, the process of linking and preparing data for analysis, the documentation of metadata, and the choice of econometric techniques and how they were applied in the analysis carried out by the OECD. It also reports on a series of robustness checks carried out to provide context on the strengths and uncertainties of the results discussed in the main report.

The Greek public employment service (PES), DYPA, has made important efforts in improving its monitoring framework to support evidence-informed policy making. It has utilised its Data Warehouse to track eight key indicators related to administrative processes and developed dashboards for visualizing data. These dashboards are accessible to both management and staff at local offices, promoting discussions and analysis. It has also made progress in the monitoring of its ALMPs, such as through a recent report tracking the employment outcomes of wage subsidy participants. The work undertaken in this project represents an important step in the process of harnessing administrative data to go beyond monitoring and conduct counterfactual impact evaluations, thus building the evidence base for more evidence-informed policymaking. It complements nicely previous related evaluations undertaken by the Foundation for Economic and Industrial Research (IOBE) and the World Bank in the context of the Elefsina pilot.

The detailed and comprehensive data available in Greece's administrative registers provide a rich basis for monitoring and evaluating its labour market and social policies. As in many other OECD and EU countries, Greece can collect and link information on jobseekers' characteristics, their participation in ALMPs, their employment outcomes and employment histories by linking PES data with employment registers. These data sources were used in the OECD CIE on training and wage subsidies and allowed a detailed analysis based on rich information on the personal characteristics of jobseekers (such as their age, education and assessed employability), their labour market outcomes (in particular employment, unemployment, earnings, days worked, occupation) and their participation in different ALMPs. Nevertheless, it is important to note that the process of constructing the data used for this analysis was complicated by some inconsistencies in the underlying administrative data which are put together for operational purposes and had to be prepared specially for research purposes. This meant that certain assumptions had to be made in order to create a unified, consistent dataset containing the labour market history of individuals registered with the PES.

[1] "Pilot studies on impact evaluation of labour market and social policies through the use of linked administrative and survey data" which is co-funded by the European Union (European Commission's Directorate General for Employment, Social Affairs and Inclusion) (VS 2020 0368).

The choice of methodology used in the OECD (2024[1]) impact evaluation was dictated by both the availability of rich administrative data and the lack of strict eligibility criteria for ALMPs. In the training and wage subsidy programmes evaluated in Greece, participation is not randomly assigned and the programmes as a whole are open to the majority of jobseekers. While specific implementations of the programmes do include strict eligibility criteria (such as age thresholds), they are implemented in parallel with similar programmes that collectively cover a wide range of jobseekers. Taking into account these multiple factors, the econometric strategy that was eventually adopted harnesses the rich individual characteristics available to compare participants with similar non-participants. This relies on using these data on observed characteristics to remove differences between participants and non-participants, closely approximating the comparison that could be made if participants had been randomly selected into the programmes.

Greece could take further steps towards more evidence-based policy making in its labour market and social policies. Specific recommendations for improvements include:

- **Incorporating additional data sources into the DYPA data warehouse** to provide the data backbone for monitoring and evaluations as well as tracking a broader array of jobseekers' labour market and social outcomes. Harnessing the wide administrative data sources available in Greece for monitoring and evaluation purposes ideally entails using datasets beyond those used for operational purposes. While transforming the operational data into a format that are more amenable for analyses requires considerable upfront investments (in terms of e.g. data processing and ICT infrastructure, to incorporate into DYPA's data warehouse), such datasets can provide synergies for further work. They can also facilitate the comparability of results from different analyses by ensuring that they originate from a consistent, common dataset that has been constructed with carefully-considered assumptions and methods.
- **Strengthening the monitoring framework based on additional data and digital tools.** The framework for monitoring ALMPs should be widened to systematically encompass all aspects of ALMP provision, measuring various dimensions to promptly identify challenges. As DYPA's technical capabilities evolve, the focus should gradually shift to ultimate outcome indicators, such as jobseekers securing suitable and sustainable employment. This enhanced monitoring information could also be incorporated into new digital counselling tools to enable counsellors to provide more individualised guidance to their clients.
- **Developing a framework for systematically conducting impact evaluations of ALMPs**. To ensure that policies are evaluated regularly and systematically requires implementing a framework for such evaluations. This may require additional financial and human resources in DYPA to be able to either conduct evaluations in-house or contract them out. It could also involve partnering with other institutions, such as the unit of Experts in Employment, Social Insurance, Welfare and Social Affairs (MEKY) within the Ministry of Labour and Social Affairs.
- **Embedding evaluation into the design of policies and programmes including, possibly through experimental approaches**. To address some of the challenges encountered in the evaluation undertaken by the OECD – most prominently, accurately measuring of counterfactual outcomes due to undeclared work – future evaluations could employ an experimental approaches, in particular randomised controlled trials (RCT). RCTs would have to be carefully implemented but would be relatively simple to analyse. Quasi-experimental approaches, as applied in the OECD evaluation, would be more simple to adopt but would ideally be planned in advance. This can be done, for example, by ensuring sufficient numbers of participants around an unemployment duration threshold triggering different in programme parameters, such as the generosity of wage subsidies, which can then be analysed.
- **Ensuring that the results of the ALMP evaluations are effectively communicated to different stakeholders**. Impact evaluations should be discussed internally, including ideally with PES counsellors, to aid their understanding of the effectiveness of ALMPs and establish broad-based

support for changes. Policymakers and the broader public should be informed to support evidence-based policies and establish the accountability of public expenditures. This is particularly salient in the context of this evaluation, which finds that both ALMPs analysed have positive effects on labour market outcomes and make a persuasive case for increasing expenditures on these programmes.

2 Establishing data availability and access required good collaboration

2.1. Introduction

Conducting a counterfactual impact evaluation using administrative data requires rich data with detailed information on jobseekers' characteristics, their participation in ALMPs, their employment outcomes, as well their employment history before entering ALMPs. Governments routinely collect such information for administrative purposes, but such data are not linked together and typically held by different institutions. Because they were not collected originally for research purposes, they are often not in a format conducive to conducting impact evaluations. Linking such administrative data together thus presents an enormous opportunity to leverage existing data for evidenced informed policy making, with many OECD and EU countries now using such data to perform impact evaluations (OECD, 2020[2]). The work undertaken in this project represents an important step in the process of harnessing administrative data to conduct CIEs and build the evidence base for more evidence-informed policymaking.

Linked administrative data form the foundation of the impact evaluations conducted in this project. These data are described at a high-level in Chapter 3 of the main report for this project (OECD, 2024[1]). To support a more comprehensive and technical understanding of these data, this chapter describes the data used, details how access to these data was facilitated, and explains how DYPA could build on this work to produce more impact evaluations of ALMPs in the future.

The rest of this chapter is structured as follows. Section 2.2 explains the process and timeline for obtaining the data. Section 2.3 describes the data and their limitations. Finally, Section 2.4 recommends ways to make the most of such data by improving access, increasing the capacity to link and analyse them and discussing additional possible administrative data sources that could further enrich the analysis.

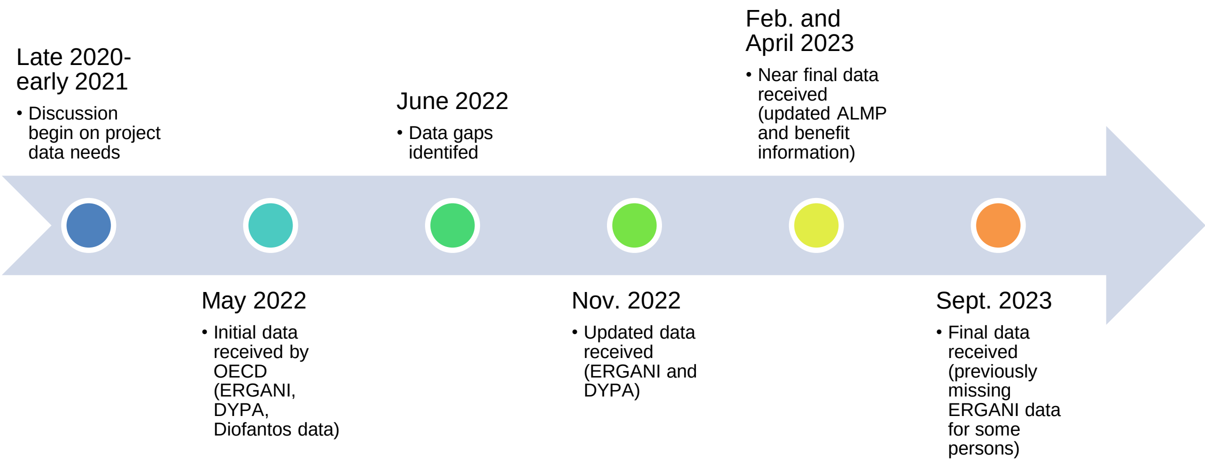
2.2. Data linking was a collective successful effort across several institutions

This project linked data from three different institutions: DYPA (information on unemployment and participation in wage subsidy programmes), ERGANI (employment data and further information on wage subsidies) and Diofantos (data on training programmes). This section describes the process of linking these data and making them available to the OECD for analysis.

Figure 2.1 shows the timeline of the data linkage process for this project. The process began with the OECD working with DYPA to ensure a common understanding of the broad types of data needed to carry out the evaluation. This process involved several discussions starting in 2020. DYPA then worked with the ERGANI team (who holds employment data) and the Diofantos team (who holds data on participation in training programmes) to communicate the data needs.

Figure 2.1. The process of acquiring the complete, final data lasted almost three years

Timeline of data linking and acquisition milestones



Discussions on the data needed first entailed establishing which data sources existed and could conceivably be used. The next step concerned determining the programmes to be analysed and the time span the programmes covered. The programmes were selected to have a follow-up period of several years during which outcomes could be tracked.

Following these consultations, it was agreed that data from the following sources would be shared:

- **DYPA,** covering unemployment history, participation in ALMPs, and benefit receipt.
- **ERGANI**, covering employment histories.
- **Diofantos**, covering training participation.

Once the programme to be analysed were identified, DYPA prepared a list of individuals for whom employment data would be requested from ERGANI. All three agencies then took steps to collate and pseudonymise the data. To protect people's anonymity, the identifiers for individuals and employers were pseudonymised. This needed to be done consistently across all tables so that data for specific individuals could be linked to form a detailed labour market history of individuals. For example, if the ID '123' is pseudonymised to the hash 'dkljhkj17y891' in the DYPA data, then '123' had to be pseudonymised to the same hash value 'dkljhkj17y891' in the ERGANI and Diofantos data. This pseudonymisation process was conducted by DYPA.

After the data were compiled and pseudonymised, they were then transferred to the OECD using secure encryption protocols. The first wave of data was received in May 2022. The OECD carried out an initial analysis of the data, identifying two major issues that needed to be addressed:

- **Problems with the coverage of the ERGANI data**: only about 20% of the individuals in the DYPA data ever appeared in the ERGANI data. While some of the unemployed may never have been employed (or at least not during the period covered by these data), it was implausible that so few people had worked during this period. This was later confirmed as a misunderstanding regarding the data required to conduct the analysis.
- **Lack of information on stock of unemployed on 1 January 2017**. DYPA data covered only new registrations since 1 January 2017, but the analysis required all “live” registrations from 1 January 2017 (i.e. not only those who registered after this date, but also those who were registered then and were still subsequently unemployed). Several other variables that were identified as beneficial

in the DYPA and ERGANI data were also flagged with the aim to have them included in the next data that would be shared with the OECD.

To address these challenges, several discussions were held, including bilateral and multilateral meetings between the OECD, ERGANI and DYPA. This also included face-to-face meetings during the OECD mission to Athens in July 2022, as well as numerous email exchanges. These efforts proved fruitful and a new round of data was provided to the OECD in November 2022. Critically, this included those jobseekers registered with DYPA at 1 January 2017 whose registration date was before 1 January 2017. Furthermore, the new data delivered significantly improved ERGANI data, which included information for many more individuals than previously identified, and included additional variables.

Some further data on ALMP participation were needed and a new round of data was delivered to the OECD in February 2023. Following the arrival of the new data in February, the OECD delivered a preliminary presentation of the results in May 2023. The swift completion of the intermediate analysis was facilitated by earlier efforts, which established a thorough understanding of the data and allowed for the pre-writing of some portions of the code in Stata (the software used to analyse the data), especially pertaining to data cleaning.

Following the presentation of preliminary results in May additional concerns were raised about missing data for some individuals. In particular, significant numbers of wage subsidy participants entering the programmes after August 2020 did not have matching employment records in ERGANI (up to 83% in ERGANI, whereas before this date almost 100% of participants in wage subsidies were correctly recorded in employment). The DYPA team identified many unemployed persons whose ERGANI data were missing. DYPA and ERGANI then worked together to provide these data to the OECD in late September 2023. This additional information provided employment information on roughly ten percent of individuals with employment records (Table B.1).

2.3. The data provided are rich, but limitations remain

This section describes in detail the data used in this project, including both the specifics of what was in the data received as well as what was not, but could have been useful. While an overview of the data was provided in Chapter 3 of OECD (2024[1]), the purpose here is to describe the data and its limitations in a way that would benefit a technical audience, including analysts who might work with similar data in future projects.

2.3.1. Data from three organisations provide a rich understanding of individuals' characteristics and outcomes

Data from DYPA, ERGANI, and Diofantos provide a rich basis for the analysis. Table 2.1 offers an overview of the data from each of the three sources: The table gives an in-depth understanding of the data's structure including the unit of observation (i.e. what the rows of the data set identify) alongside the sample provided and an extensive list of variables in each dataset. It is hoped that this table can act as a source of basic metadata for others (especially researchers and analytical staff) and prove useful for potential future projects.

The core sample of the study consisted of all registered unemployed persons between January 2017 and August 2022 (2 566 735 persons with a total of 5.8 million unemployment spells). Note that this includes all people who become unemployed during this period, as well as those who became unemployed before this period and were still unemployed as of 1 January 2017. The ERGANI data, which spans March 2013 to August 2022, then covers records related to these persons but does not cover those persons who were never registered unemployed (i.e. if a person has not been unemployed they are not covered in the ERGANI data collected for this project, likewise persons who have been unemployed but never employed will not appear in ERGANI data either). Diofantos data cover all participants all seven training programmes for unemployed persons between 2016-22. In consultation with DYPA, it was decided to evaluate three of these programmes.

Table 2.1. Linked administrative data used in this project

Data source Dataset(s)	Unit of observation	Sample	Variables
DYPA (Greek public employment service) unemployment spells	Unemployment spells	All persons registered as unemployed with the PES at some point from January 2017 and December 2021	Person ID, registration number, registration start, registration end, occupation code, previous job end reason, previous job end date, occupation type, occupation kind, special category (people from various vulnerable group, including immigrants, asylum seekers and refugees, young offenders, homeless persons, people with disabilities), unemployment end reason, whether profiling is performed, suggested profiling score, final profiling score
DYPA personal characteristics of unemployed	Individuals	All persons unemployed at some point between January 2017 and August 2022	Person ID, gender, Birth year, nationality, citizenship, education level, family status, number of children, years on unemployment register, municipality (325 municipalities), region (13 regions), foreign language, has computer skills, has driver's license
DYPA ALMP participation	ALMP participations (in rare cases multiple participations per person)	Unemployed persons participating in ALMPs from March 2017 until (approximately) end 2022	Person ID, employer ID, prefecture, regional unit, DYPA regional office, DYPA sub-regional office, programme title, financial activity code, financial activity, person start date, job id, date job was advertised
DYPA unemployment benefits data	Unemployment benefit spells	All persons unemployed at some point between January 2017 and approximately late 2022. Benefit data for these persons covers from 2013 to early 2023.	Person ID, start date, benefit type (unemployment and related benefits), number of days benefit paid, benefit amount
Diofantos (separate dataset for each programme)	Detailed information on training	Persons participating in training programmes (seven programmes between 2016-21)	Person ID, date of birth, sex, activated training voucher, education, municipality, training subject, training department, provider name, provider certificate, provider type, provider management structure, provider city, start date theory component, end date theory component, successful completion of theoretical training based on absences, exam success (pass/fail), hours of theoretical component, start date internship component, end date internship component, internship firm ID, internship business, internship city, internship successful absence, benefit paid theory, benefit paid internship, professional status after programme, business address after, type of job after programme, internship hours
ERGANI employment data (separate datasets for each year, and whether contract is for internships, regular contracts or public works)	Changes to employment conditions (i.e. separate row for hiring/separation but also for when employment conditions change)	Individuals who were both: • Employed at some point during the March 2013 – August 2022 AND • who were unemployed at some point during 2017-21	Person ID, employer ID, action type (e.g. hiring, separation, change of contract), first time employee, employee type, hours, full time/part-time, special case (internship, wider public sector) , hourly wages, occupation, contract type, experience, branch, DYPA referral, employment programme, if the person is replacing a previous wage subsidy participant (and if so who they replaced), unemployment benefit, if receiving unemployment benefits from which DYPA branch, NACE code of the employer, municipality, employer legal status
ERGANI firm level information	Employers	Employers of individuals that employment data was provided for	Employer ID, total number of persons working at the firm (including also individuals never registered with the PES for whom we did not get individual-level employment records above)

Source: OECD compilation based on data from DYPA, Diofantos, and ERGANI.

2.3.2. Several limitations in the existing data could be addressed for future evaluations

While in general, the data used in the impact evaluation were comprehensive and detailed, several limitations are worth mentioning. These could be especially useful in subsequent impact evaluations.

The following limitations have been identified in the unemployment registry data received from DYPA:

- **Variables in the DYPA personal characteristics of unemployed" are a snapshot at one point in time only** (understood to be the most recent registration). While this is not problematic for variables that are time invariant in principle (such as gender, year of birth, nationality), it is less than ideal for variables that do change over time, such as education, family status, region, and languages. This is probably still not a major concern as such variables likely do not change often, especially for non-youth who will have completed studies.
- **Missing information for additional skills in the unemployment register, such as computer skills and driver license variables**: It is optional for jobseekers to add such skills to their profile and as such these variables are missing in more than 80% of cases in the raw data. Thus, there is likely a strong selection effect for reporting such skills. Econometrically speaking these variables are included in the estimated propensity score models as dummy variables to account both for the presence of such skills, or for whether the value is missing. Such an econometric approach allows for taking advantage of the information when it is provided without having to drop missing observations from the analysis**.**

The data on wage subsidy participation had the following limitations:

- **Lack of information on which programme parameters applied to each participant.** Many of the programmes specified different parameters, such as requirements for retaining workers after the end of subsidised employment periods, which were contingent on the characteristics of the individual. While these parameters could sometimes be inferred from the unemployment register data – for example, based on their unemployment duration upon entering the programme – it would be helpful to have precise information on this. This would be particularly useful for analysing the role of programme parameters in a programme's effectiveness, though a regression discontinuity design. For such evaluations, it is especially critical to have the precise information, as individuals are analysed close to the threshold (e.g., programme parameters in several programmes were different for individuals before and after 12 months of registered unemployment). In the absence of specific information, there is a potential for errors to be introduced. For example, an individual may have applied to enter a wage subsidy programme at 11 months of unemployment duration and began the programme more than 12 months after registering as unemployed.
- **Lack of precise data on end dates.** Information on wage subsidy duration could help in the calculation of unsubsidised employment duration. Without such end dates, the analysis rests on estimates of wage subsidy duration inferred from the programme parameters and jobseeker attributes. However, as discussed in the previous point, this may not be measured precisely.
- **Lack of information on programme costs.** To a certain extent, programme costs could be inferred on an individual-level basis. However, given the multitude of factors in the calculations – including specific ceilings on payments in certain programmes – any such calculations could be subject to considerable uncertainty.
- **Lack of consistency across data sources, pointing to possible issues with data reliability.** Both DYPA and ERGANI provided information on participation in wage subsidy programmes. However, there was not a perfect alignment between the data from these two sources. DYPA recommended relying on their data as it is considered more reliable for several reasons. Firstly, not all wage subsidy programmes require a contract type to be recorded in ERGANI. Secondly, there is often confusion in ERGANI forms because different programmes have similar names,

leading to mix-ups. Due to these factors, the analysis primarily depended on DYPA's data to assess wage subsidy participation.

The ERGANI employment data had several limitations:

- **Accuracy issues with ERGANI hours and wage data**: These data are known to be especially problematic and contain many errors prior to October 2016. After this date, administrative changes to how they were measured has been said to improve the values. This makes historic measures of hourly wages hard to observe. Such measurement error is far from ideal. During the period for which these variables are known to be most problematic they are used (after transformation, see Chapter 3) as covariates in the propensity score models. Measurement error in this context can cause the estimates to be less precise (wider confidence intervals) and biased. Indeed even the direction of such bias is difficult to ascertain as it depends on whether the measurement error is random (in general non-random measurement error can be expected to be more problematic), and the specific relationship between the mis-measured covariates (past hours and wages) and the probability of treatment or the outcome (future hours and wages) (Millimet, 2011[3]; Bound, Brown and Mathiowetz, 2001[4]; Levi, 1973[5]).
- **Lack of consistent data on contract end dates**: The ERGANI dataset occasionally includes instances where a person's job commencement is recorded, but there is no corresponding record of job termination. To address this, an annual census of employment in firms is utilised, where the absence of an individual in the census implies the end of their employment (Section 3.1 provides more details on the approach adopted). However, two significant limitations exist: firstly, the exact month of employment termination cannot be pinpointed, as the census is conducted only once a year in October. Secondly, inaccuracies in the census data, such as failing to report an employee's presence in a firm, can lead to the premature conclusion of an employment spell.
- **Lack of data on public sector employment.** Data on public employment are not covered in ERGANI, as the system was implemented in part to facilitate enforcement of legal employment in the private sector. This limitation is not overly concerning, as the wage subsidies evaluated are designed to encourage private sector employment, and the selected training programmes do not aim at public sector job placements. Additionally, consultations with stakeholders indicated that public sector hiring was minimal during the study period. Therefore, the impact evaluation has likely not overlooked any substantial employment outcomes in the public sector due to this data exclusion. For future evaluations, incorporating data on public sector employment could be valuable, particularly if there is an increase in public sector hiring.

The final data source used in the analyses – the data on training from Diofantos – are comprehensive and did not have major limitations for this study. However, there was a minor limitation with the data in the form it was shared with the OECD. This relates to the employer identifiers for the practical component of the training (note the programmes contained a mix of both classroom-based and practical training). Specifically, the identification number of the employer was not provided in a form that could be linked with the data in ERGANI. This meant that it was not possible to use ERGANI data to track if people stayed on with the same employer where they conducted the practical training. This, however, is not a major limitation to this study but it could be useful to address it for future evaluations.

2.4. Suggestions for improving the data acquisition and analysis process

A growing body of labour market research in Greece has been able to make use of linked ERGANI, DYPA, and Diofantos data. This includes not only the current study evaluating training and employment subsides in Greece but also the OECD's work examining unemployment benefit reforms in Greece (OECD, forthcoming[6]), the World Bank's work on the Elefsina pilot (World Bank, 2021[7]), IOBE's work on impact evaluations of wider vocational education and training programmes in Greece (IOBE, 2021[8]), as well as DYPA's own evaluations of its wage subsidy programmes (IOBE, 2021[8]).

These projects together demonstrate the value of linking administrative data in Greece for research purposes. They also highlight the absence of technical obstacles to compiling such databases on an *ad-hoc* basis. However, there is scope for improvement in making the data linking process more efficient and streamlined for all parties involved.

2.4.1. Linking data takes time and effort – especially the first time it is done

The sharing of such data involves both technical requirements and the need for cooperation. It demands resources and commitment from various individuals across different organisations, which can be time-consuming. Since these projects are not routine, the required resources are not always readily available, leading to delays. These delays are often exacerbated because many steps in the process must be completed in a specific order and cannot be done in parallel. For instance, the team handling the ERGANI data first had to receive a list of IDs for unemployed individuals from DYPA before proceeding with their data extraction. Although this project successfully navigated these data linking challenges, the process of acquiring the complete and final data ultimately took several years.

Organisations that frequently link data can maintain the necessary capacity, simplifying subsequent data extractions. Some parts of this process, such as data anonymisation and extraction of specific variables, could potentially be automated if repeated for newer datasets. Regular data exchange could streamline the process. Once linked, the data can be efficiently utilised for multiple purposes (provided the purposes are aligned with a legal basis to use the data as such). The data linkage facilitated by this project was in fact employed for two different OECD studies, reducing the burden of providing the data from the Greek authorities.

2.4.2. Establishing a repository of metadata would facilitate subsequent evaluations

Having a good understanding of the data is a precondition for any analysis. This project benefited enormously from fact-finding missions, online meetings and email exchanges to better understand the data. However, these communication methods are time consuming for all parties involved. This technical report encapsulates some of this acquired knowledge to be used in future research. Nevertheless, future projects may have different objectives and research questions, leading to further queries about the administrative data sources. The compilation of detailed metadata and data dictionaries would be extremely beneficial to both researchers and the administrative data providers, streamlining the research process by reducing the need for ad hoc responses from administrative data providers in response to queries. This technical report aims to provide the basis for metadate and dictionaries that DYPA or MEKY could develop going forward.

2.4.3. Allocate more resources to analysing linked administrative data including for impact evaluation

There are many questions that can be addressed with linked administrative data. However, answering these questions well requires devoting sufficient resources to such evaluations. The decision on whether to conduct CIEs internally or via external contractors varies across countries, often involving a blend of both approaches. This choice is shaped by several factors, including the required expertise for the analysis, the feasibility of sharing data with external entities, the frequency of evaluations, and the management of contractors and their reports. Countries lacking in-house analytical capabilities are more likely to benefit from outsourcing research (an approach adopted, for example, by Finland and detailed in OECD (2023[9])). An alternative approach is to develop a specialised in-house evaluation team (an approach adopted, for example, by Canada, and detailed in OECD (2022[10])). Regardless of the approach chosen, DYPA should consider hiring more staff to expand the capacity it has in this area. Even if CIEs are contracted out, having staff with the requisite knowledge is important in order for them to serve as an intermediary between policymakers and researchers as well as to competently contract out the analyses. Collaborations with universities could help DYPA, in the medium term, to build its capacity to use data for research purposes and answer some relevant policy questions.

Furthermore, advancements in digital technology have enabled some types of impact evaluations to be automated and conducted with minimal staffing resources (OECD, 2022[11]). For example, the Estonian PES has automated ALMP impact evaluations using statistical software and Business Intelligence tools, effectively visualizing labour market effects of various programmes and schemes with near-live Data Warehouse inputs. Similarly, the Slovak Republic has been publishing automated ALMP impact reports since early 2022, developed externally using data from both the Slovak PES and the European Commission's Labour Market Policy Database. The German PES, a pioneer in this field, employs a semi-automated tool called TrEffeR for similar evaluations, though it is not fully automated and is updated twice yearly.

2.4.4. Consider linking additional information from other government agencies to consider broader outcomes and conduct cost-benefit analyses

DYPA's digital capabilities have significantly advanced with the introduction of new data exchange options through the Central Interoperability Centre (KED). This development has enabled the establishment of web services for sharing information on various aspects such as employment records, taxes, social security, property, and refugee status. The current challenge is harnessing the potential offered by this new platform for analytical purposes, while adequately addressing questions relating to data confidentiality and security, and establishing any necessary legal protocols for their use for these purposes.

Incorporating additional data sources into analyses would allow DYPA to examine the effects of ALMPs on broader outcomes: an even richer set of labour market outcomes as well as additional ones, such as those relating to social inclusion or health. For example, while these questions have been insufficiently subject to rigorous CIE, it could be the case that ALMPs improve health outcomes (unemployment has been shown to adversely affect health), reduce crime, or lead to increased social inclusion (especially for ALMPs targeted towards the most vulnerable).

Future impact evaluations could also conduct cost-benefit analyses, which could strengthen the business case for increasing expenditures on ALMPs or inform the allocation of funds across programmes. Cost-benefit analyses could also include estimates of other social benefits. These include quantifiable benefits that cannot be expressed in monetary terms, as well as qualitative benefits that may be difficult to quantify (HM Treasury, 2022[12]). Such benefits include, for example, non-monetary benefits arising from individuals participating in the labour market, such as strengthening social ties and gaining a sense of independence or self-worth from doing meaningful work. Nevertheless, taking into account such costs and benefits can facilitate evidence-informed policymaking.

A final benefit from brining in additional information on the unemployed is that it could further improve the accuracy of the CIEs. This is because quasi-experimental methods (such as the method employed in this project) rely on establishing treatment and control groups that are as similar as possible to each other. This requires a rich set of data, and while the current project demonstrates that this is possible, bringing in even more information on unemployed persons could further bolster confidence that the treatment and comparison groups are similar.

3 Data linking and preparation involved many steps

This chapter discusses the process of linking, cleaning, and shaping the data files provided to the OECD by the Greek authorities to make them ready for the OECD (2024[1]) impact evaluation. It also compares the statistics calculated from the individual-level data with comparable statistics from publicly-available sources.

3.1. The process of data cleaning and linking required reconciling different data sources

Creating datasets usable for evaluation purposes required restructuring and linking several datasets that are structured differently at the outset. In the Greek data, the unit of observation differed across the data files, requiring some data processing to ensure that they could be compiled into a joint two-dimensional dataset for the analysis. As discussed in the previous chapter, these various databases were then merged using pseudonymised individual identifiers.

In terms of the sequence of processing, each dataset first underwent a series of consistency checks and was then merged together with the most closely related dataset, undergoing another set of consistency checks before being merged with another dataset. The data from the unemployment registry was merged first with the unemployment benefit data, and the resulting combined dataset merged subsequently with the employment data. These were then merged together with the ALMP data. The data were then reshaped into monthly data.

3.1.1. Compiling a consistent employment dataset involved multiple steps and assumptions

Considerable data processing was required to compile a unified dataset on employment from the ERGANI data. The basic challenge was related to the fact that the ERGANI data were provided from the operational database, which records relevant changes to employment as they arise. Table 3.1 summarises the key features of the type of changes recorded in the ERGANI data and their observed frequency in the data received.

Table 3.1. Compiling the employment data required joining separate observations into coherent employment spells

Forms used to compile employment data and their observed frequency in raw ERGANI data

Form Name	Purpose	Information reported	Reporting frequency	Number of observations in raw data	Percentage of total observations
E3	Unified Form for Announcement of	Employee details, job description, employment	When hiring new employees	12 944 524	36.22%

	Hiring	terms			
E4	Personnel Table (Various Types)	Employee roster, work schedules, earnings, employment changes	Annually, initially, or when changes occur	7 384 983 (Annual census) 3 174 555 (Earnings changed)	20.66% (Annual census) 8.88% (Earnings changed)
E5	Announcement of Voluntary Departure of an Employee	Employee's personal details, departure date, reasons for leaving (if applicable)	When an employee voluntarily leaves the job	4 181 262	11.70%
E6	Termination of Indefinite Employment Contract	Employee details, termination date, reasons for termination (with or without notice)	When terminating an indefinite employment contract	34 662 (Dismissal) 19 445 (Dismissed – Warned) 2 377 679 (Dismissed – Not Warned)	0.10% (Dismissal) 0.05% (Dismissed – Warned) 6.65% (Dismissed – Not Warned)
E7	Certification for Fixed-term Contracts or Work	Details of fixed-term contracts or work, including duration and nature of work	For fixed-term contracts or specific work assignments	5 625 242	15.74%

Source: Official Government Gazette (2019[13]) and OECD calculations based on data from ERGANI.

Compiling an analysis-ready employment dataset required consolidating and transforming ERGANI data into the data to define an individual's employment spells – periods of continuous employment – at a given employer. This involved restructuring the data into a format in which one observation referred to the attributes of an employment contract within a specific period of time. In this dataset, subsequent changes to this employment contract – for example, changes to the wages – are recorded as a separate observation with a corresponding time period (measured in specific calendar dates). The Stata code developed for the analysis defined employment spells by generating "from" and "until" variables, representing the start and end dates of these employment periods. This involved identifying and prioritizing certain types of employment actions (e.g., hiring, census, earnings changes) and handling cases with multiple actions on the same date. Additional data cleaning included consolidating overlapping employment spells, handling duplicates, and restructuring data for further analysis. Several assumptions were made in this process:

- Where multiple employment actions occurred on the same date, the script assumed a hierarchy of importance (e.g., giving priority to hiring actions over census updates). For example, if records from hiring (E3), annual census (E4) or earnings changed (E4) occurred on the on same date, priority was given to E3 because it contained more information. If any of the separations were recorded on the same date, priority was given to E5 (resignation) over others. These issues were present in 1.4% and 0.3% of observations in the raw data, respectively.
- In case of overlapping spells within a given employer, longer spells were given priority. This issue was present in roughly 0.14% of observations.
- Fixing inconsistencies, such as employment start dates that were later than end dates, also present in 0.14% of observations.

Merging the unemployment and employment datasets required considerable cleaning as well. Most prominently, this required reconciling overlaps in the dates (present in 1.2% of observations in the raw data).[2] This was resolved by giving precedence to the later spell, adjusting the end dates of the first spell in the process. Consultations with DYPA indicated that sometimes individuals' unemployment end dates were set in advance for a predetermined period which, in some cases, may not have been updated if an individual became employed – explaining the source of the inconsistency.

[2] Before merging the data from the employment and unemployment registries, a small number of overlapping unemployment spells needed to be resolved in the raw unemployment registry data. These occurred in 0.12% of observations.

An additional relevant step in determining employment dates was to impute the end of spells for individuals without period updates from the annual census. Employers most commonly submit information from the annual census of employees in October of each year, but this can also vary in the data. For this reason, a cut-off date was used to impute the end of employment spells: if individuals were not observed to have an ERGANI record within 396 days, the employment spell was assumed to have finished 396 days after the last record. This period was chosen because it corresponds to roughly 13 months and the 95th percentile of duration between two observations referring to the annual census (E4 forms). This adjustment had to be made in 0.9% of observations.

Further data cleaning was required to ensure consistency and remove implausible values in the data on earnings. A multi-step algorithm was used in the process. First, it computed imputed earnings per day for observations marked as outliers. This was done by taking the earnings per day from the previous non-outlier record or, if necessary, from further back in the employment history. If the imputed earnings remained missing after the above steps, the algorithm assigned the mean earnings per day for that specific occupational group and duration category The final earnings for each observation were then recalculated, where necessary, using the imputed earnings per day values multiplied by the duration of the employment spell. Although the number of observations affected by the earnings imputations was relatively small (0.18%), it was an important step because outliers could have a disproportionally large effect on biasing estimates of earnings.

3.1.2. The final data processing step involved reshaping the data into a monthly panel data format

The most fundamental question faced in terms of shaping the data was how to combine data in spells format with data in panel format. Spells data record only such information as the start and end date of an ALMP measure. Panel data record individuals' status in every period, for example every month.

The data used in the impact evaluation recast all of the data in monthly panel format to ensure that information from all data sources could be combined. In part, this was a question of precision: it is more straightforward to convert data in which the timing is recorded more precisely (for example, the precise date) to a data format in which timing is recorded less precisely (for example, at the monthly level). However, having the data stored in a monthly panel is also more suitable for the econometric approaches described in Section 4.5. In practice, the econometric approaches often required a special wide panel dataset to be created, where – rather than the time variable being calendar month per se – the time variable effectively captured both time spent in unemployment (labelled m in Section 4.5) and the time elapsed since the start of the ALMP measure or the start of eligibility (labelled t).[3]

Even after deciding to place the data in panel format, there remains the additional question of how to aggregate the data so that the unit of observation is an individual unemployed person. This question arises because each individual may in theory participate in more than one ALMP measure, have more than one concurrent employment, and have more than one spell in unemployment. The option selected in the case of Greece was to retain information from all sources and organise the data in a so-called wide format. In the case of concurrent employment spells, total earnings were aggregated at the monthly level and this value was used for the analysis pertaining to earnings. For the other attributes such as occupation, the "main job" was identified as that one with the largest share of earnings for that individual in that month.

[3] This special wide panel dataset was created from the panel dataset, with additional variables in the wide dataset relating to outcomes at different time periods relative to the reference point in the original panel dataset.

3.2. Statistics on participation and number of registered unemployed

The process described above, as well as the matching process described in the next chapter, resulted in a small number of observations being excluded from the final sample (Table 3.2). The process of data cleaning and retaining only individuals from the first time they entered an ALMP resulted, respectively, in 3.3% and 2.0% of observations in training and wage subsidies being dropped. A larger share of individuals were not included in the final analysis in the case of wage subsidies due to a lack of suitable matches: 14.1% of the total sample (the respective share of training participants was only 0.1%). This is at least partly due to the larger total number of wage subsidy participants, which made it more likely that a suitable match could not be identified.

Table 3.2. Not all participation in the programmes analysed was included in the final analysis

Attrition of observed entrants into training or wage subsidy programmes,[1] Greece

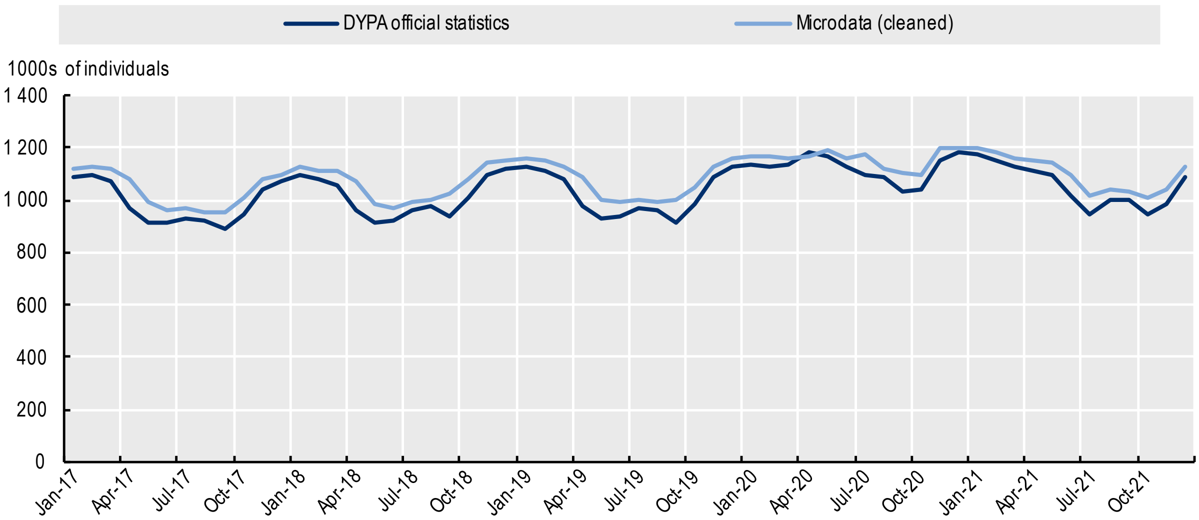
	Training programmes	Wage subsidy programmes
Raw data	22 873	65 269
After combining data sources and keeping only first ALMP participation in spell	22 116	63 954
After matching (final sample)		
- Caliper=0.05 (for reference)[2]	22 115	56 419
- Caliper=0.01 (used in analysis)[2]	22 102	54 746

Note:
1. Refers to all participants in the programmes selected to be examined in the impact evaluation, entering between 1 March 2017 and 31 July 2021.
2. The caliper refers to the size of the restriction placed on the maximum allowable distance between matched units. This concept is often used to improve the quality of matching and ensure that the matched units are similar to each other in terms of the variables being considered. In propensity score matching, a caliper ensures that the propensity scores of individuals being matched are not too far apart.
Source: OECD calculations based on data from the Greek public employment service (DYPA), ERGANI and Diofantos.

The resulting database contains detailed information on the 2 578 038 unique individuals who were registered as unemployed at any point during the 2017-21 period. Comparing the monthly stocks of individuals in this dataset with the official statistics on the number of registered unemployment shows a small discrepancy averaging roughly 6 thousand individuals over time, with more individuals observed in the microdata (Figure 3.1). Both data sources display a strong cyclicality in unemployment stocks within calendar years, reflecting the somewhat seasonal nature of unemployment in Greece.

Figure 3.1. Statistics calculated from individual-level data are comparable to official statistics

Monthly stocks of registered unemployed, Greece



Note: Statistics from microdata refer to the final analysis dataset after consistency checks and data cleaning.
Source: OECD calculations based on data from the Greek public employment service (DYPA).

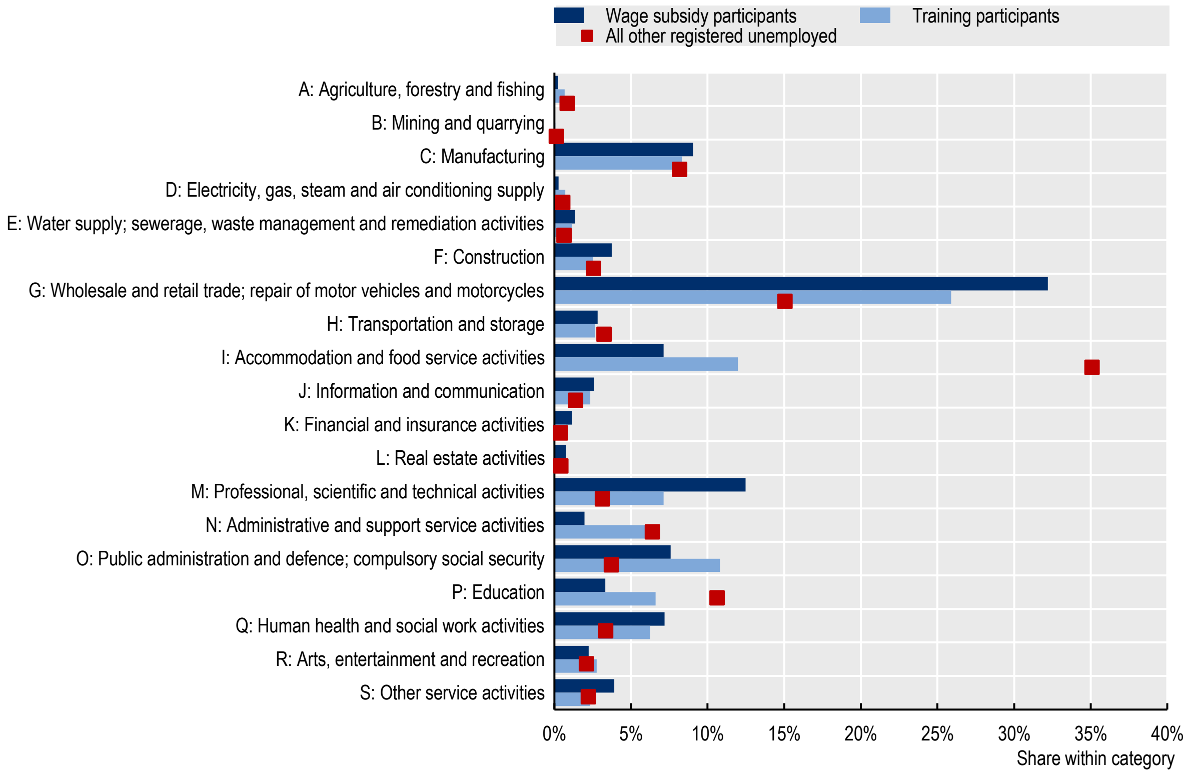
3.3. Additional summary statistics of participating firms

This section examines some additional characteristics of firms who hire the participants (either directly during the wage subsidy period or after the end of their training). To provide a sense for which sectors of economic activity hire jobseekers entering ALMPs, Figure 3.2 contrasts the characteristics of the participants in the selected training and wage subsidy programmes with the characteristics of all individuals who are hired into these sectors from among those registered as unemployed with the PES. In the case of the wage subsidy, the figure depicts employment during the subsidy period. For training, the figure depicts the first job for individuals who become employed after the end of the training programme. Alignment or non-alignment of the bars with the red squares indicates whether the ALMP participants are overrepresented or underrepresented in a sector compared to their overall distribution among the registered unemployed. For example, a longer blue bar than the corresponding red square in a sector would suggest that training participants are overrepresented in that sector compared to hires among the general unemployed population.

Sectors with disproportionally high hiring rates of ALMP participants include the wholesale and retail trade sector as well as the professional, scientific and technical activities sector. Interestingly, accommodation and food service activities account for a disproportionally small share of all participants. The two sectors with the disproportionally high hiring rates also tend to make hire a larger share of wage subsidy participants, while in the accommodation and food service activities tend to hire a larger share of (former) training participants.

Figure 3.2. Employers in sectors such as retail trade hire large shares of ALMP participants

Structure of hires across ALMP participants and registered unemployed by sector of economic activity, Greece



Note: ALMPs stand for the training and wage subsidy programmes evaluated in this report. Shares are calculated within each group separately: if a sector hired ALMP participants in the same proportion as their new hires amongst all registered unemployed, the length of the bars would coincide with the red squares. Statistics for stocks of all unemployed are calculated based on averages of monthly statistics during the 2017-21 period. Participant numbers refer to totals during the 2017-21 period for individuals entering either training or wage subsidies.
Source: OECD calculations based on data from the Greek public employment service (DYPA), ERGANI and Diofantos.

A related question pertains to the size distribution of employers who hire ALMP participants. As discussed in the main report (OECD, 2024[1]), larger employers disproportionally hire individuals who participated in training, whereas smaller ones hire individuals using wage subsidies. One limiting factor for hiring workers for larger employers relates to ceilings on the receipt of state aid, which are imposed by the EU. Consultations with stakeholders indicate this is one factor contributing to lower take-up rates among larger employers. Such ceilings were especially binding after the onset of the COVID-19 crisis in 2020: support to employers was included in the ceilings. However, examining the distribution of take-up by employer size shows that this was feature preceded the COVID-19 crisis. Examining the distribution of wage subsidy take-up rates across employer size shows that prior to March 2020, larger employers were even *less* likely to use wage subsidies (Figure A.1).

4 The impact evaluation methodology pairs programme participants with similar non-participants

This chapter describes the research challenges faced in identifying the effects of the two sets of ALMPs studied in the OECD (2024[1]) evaluation, the outcomes examined and the econometric approach used. It first outlines the scope of the evaluation and provides details on the outcomes measured, including the construction of the index used to measure occupational mobility. It then describes the challenges that need to be addressed in order to accurately identify the impacts and describes the econometric approach. These include considerations related to minimising bias in the estimates, such as accounting for unobserved heterogeneity between individuals in the treatment and comparison groups.

4.1. A results chain framework can help clarify different aspects of the programmes for monitoring and evaluation

Impact evaluations are essential for the development of evidence-based policy, as they provide information on the effectiveness of policies in achieving specific objectives. In order to determine what questions an evaluation should answer, it is important to first understand the anticipated causal mechanism behind the policies, to precisely identify how a policy is supposed to deliver the desired results. More specifically, this means determining what the inputs, activities, outputs and outcomes of the programmes are (Gertler et al., 2016[14]). In the context of the impact evaluation for Greece, a clear understanding of such a sequence is useful for clarifying the scope of the evaluation, as the ultimate focus is on the evaluation of results and not on inputs, activities and outputs.

In the context of this study, the “results chain” framework can be defined as follows:

- **Inputs**: the resources available for the training and wage subsidy programmes. These include funds used for training programmes, which were directly administered by the Ministry of Labour and Social Affairs for the programmes being evaluated, as well as the funds for the wage subsidies. These also include resources used by PES staff and resources to administer the programmes.
- **Activities**: activities that convert inputs into outputs of the programmes. These include the classroom- and workplace-based training programmes (in the case of training) and the work conducted during the course of wage subsidies. They also include activities conducted by the PES, including for example the on-site monitoring of employers receiving wage subsidies to verify compliance with the programme requirements.
- **Outputs**: tangible goods and services generated by programme activities. These include the number of individuals who completed training (for training), or remained employed for at least a pre-specified duration (for wage subsidies). They also include any certificates of training (in the case of some types of training). In the case of the wage subsidies studied (which were directly administered by the PES), the outputs of the programmes were at least indirectly under the control

of DYPA, through e.g. the design of the programmes' parameters. The new training programmes, implemented by DYPA since 2022, attempt to incentivise providers to improve the programmes' outputs by tying part of the payment to providers to successful completion of the training.

- **Net outcomes (impacts):** the effects that the programme achieves after the target population has received or been exposed to the programmes' outputs and activities, after taking into account counterfactual outcomes of participants had they not participated. These effects can be measured along different dimensions; the ones examined in this study are detailed in the next sections.

CIEs focus only on the last element of the results chain – the net outcomes. The elements proceeding it are discussed only to the extent that they help understand the outcomes. This makes it distinct from a process evaluation, which would examine whether programme activities have been implemented as intended. In addition, outcomes are measured as net outcomes - after taking account of the counterfactual - rather than gross outcomes, as are the current outcome indicators in the recent DYPA study (DYPA, 2023[15]). Gross outcomes would, for example, measure the employment rates of participants without taking into account that some participants would have been employed anyway (for more details on the discussion of the counterfactual approach, see Section 4.4). The outcomes examined in the OECD CIE are described in more detail in the following sections.

4.2. Several outcomes were evaluated

The rich data available for Greece enable the analysis to track a wide set of outcomes in evaluating the programmes studied and over a relatively long period. The outcomes are tracked continuously over up to the three-year period starting with the beginning of the participation in a programme. Outcome values are calculated on a monthly basis and tracked over time relative to a reference month, which is defined either as the month when an individual enters training or wage subsidies (for the treatment group) or that same calendar month for an individual in the comparison group who is matched to someone in the treatment group.

The research questions examined in the OECD (2024[1]) impact evaluation relate to labour market outcomes at time horizons from 1 to 36 months after entering the programmes. In terms of labour market states, the following outcomes are examined:

- **Probability of entering private-sector employment**. This probability is measured using a binary outcome variable that is equal to 1 if individual is employed for any amount of time during a calendar month, and equal to 0 otherwise, in private-sector employment that is recorded in the ERGANI system. The definition of employment includes various types of employment: this includes individuals on regular, open-ended contracts, but it also includes individuals on fixed-term contracts, as well as individuals in other forms of contracts registered in ERGANI (most importantly, public works, but also internships and apprenticeships).
- **Probability of receiving unemployment benefits**. This probability is also measured as binary outcome variable that is equal to 1 if an individual received unemployment benefits for any part of the calendar month. The data contain information on all the various types of unemployment benefits: in addition to the regular unemployment benefit, they include benefits such as the benefit for seasonal workers and the benefit for the long-term unemployed.[4]
- **Probability of being in registered unemployment**. This probability is also measured a binary outcome variable that is equal to 1 if an individual was registered as unemployed at any point in a calendar month.

[4] They also include benefits targeted at workers in particular occupations, such as tour guides, private nurses and forest workers.

- **Probability of being in inactivity**. This probability is also measured a binary outcome variable that is a residual term based on whether individuals who were previously registered as unemployed have, in a given calendar month, not registered as unemployed, employed or undergoing training.
- **Probability of being in unsubsidised employment**. This probability is measured using a binary outcome variable that is equal to 1 if an individual is employed for any amount of time during a calendar month, and equal to 0 otherwise, in private-sector employment that is recorded in the ERGANI system, except in cases where the individual is presumed to be in subsidised employment. The latter is estimated by taking programme parameters (see Table B.2) and applying them to the individual's characteristics, if relevant. Instances where individuals' subsidy receipt was extended are also observed in the data and taken into account.

In addition, the following labour market outcomes are also analysed:

- **Cumulative employment duration**. This measures the cumulative duration of all jobs held in the private sector (and recorded in ERGAN) during the observation time, after the reference month. This measure is calculated on a monthly basis as the number of calendar days an individual was registered as employed based on observed employment spells. It takes without modifications instances where individuals were registered as employed for several days at a time, often in short succession, at a given employer.
- **Occupational mobility**. The analysis maps the occupation of individuals entering employment onto an occupational index, which can be interpreted as a "job ladder". By construction, the index is set to equal to 100 for the average real wage observed in the data (where the data contains employment only on individuals who were unemployed during the period under study). Changes to the index can thus be interpreted as changes in average earnings associated with an occupation, in percentage points relative the average wage earned by individuals who were every unemployed. The construction of the index is detailed in Section 4.3.
- **Cumulative earnings**. This measures total earnings, gross of income taxes and employee contributions, in constant 2015 prices, employers have indicated in the ERGANI system for all employment registered in ERGANI during the observation time. Strictly speaking, the information pertains to "advance estimates" – what the employer anticipates paying based on e.g. the individual's hourly pay. Actual payments are recorded in the tax register and may differ from what is recorded in ERGANI due to, for example, differences in actual hours worked by an individual. Employer's contributions are not included in this amount. The conversion from nominal into 2015 prices is done based on the monthly HICP index as reported by Eurostat (2023[16]).

Taken together, the outcomes provide a rich and nuanced picture on the effects of ALMPs on the participants. This comprehensive approach allows a deeper understanding of the impacts of the ALMPs, revealing not only employment trends but also patterns of earnings, occupational shifts and longer-term labour market trajectories, which are helpful for informing policy decisions and refining ALMPs.

4.3. An index was constructed to measure occupational mobility

In addition to analysing outcomes typically examined in CIEs of ALMPs, such as employment probability or earnings, OECD (2024[1])aims to address another important question: the effect of participation in ALMPs on occupational mobility. In order to provide a tractable measure of occupational mobility, the analysis relies on an occupational index, which is calculated from observed wages. Following the approach adopted by Laporšek et al. (2021[17]), a wage index is calculated for each detailed occupational code using data on the wages and employment of all individual who were ever unemployed in Greece during the 2017-21 period. This index maps each of the 1 649 distinct occupational codes observed in the data into an index that has an intuitive and practical interpretation: an occupation whose index value is one unit greater than another occupation's index value has an average real monthly wage that is one percentage point higher relative to the minimum wage. Furthermore, increases and decreases in the index can be interpreted, respectively, as positive and negative changes in an individual's occupation: climbing up or down the occupational ladder.

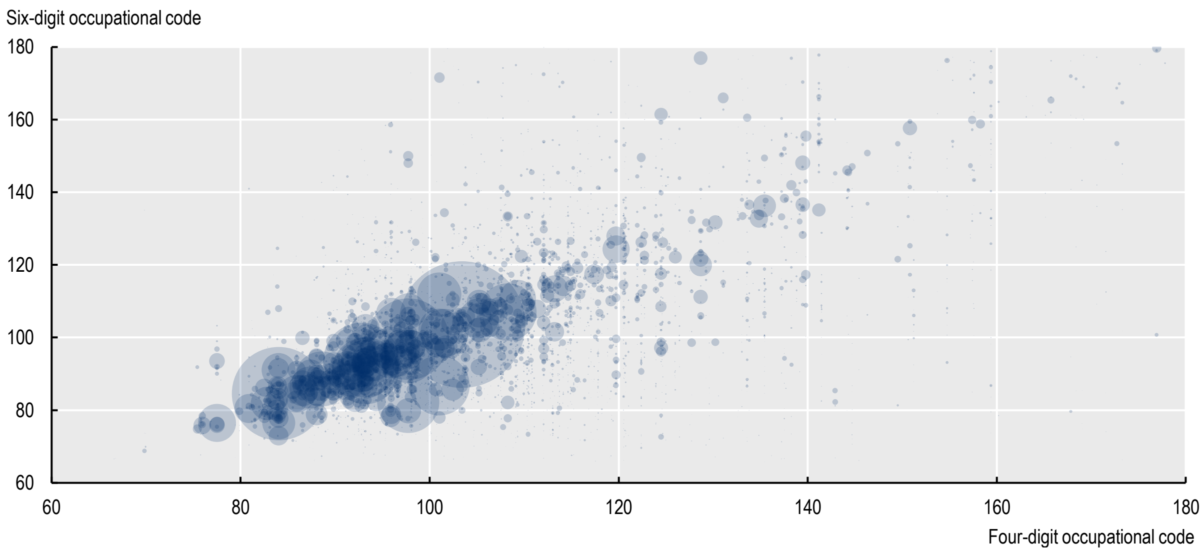
The analysis uses 6-digit occupational codes calculated from real monthly wages at constant 2015 prices. The classification uses Greece's national classification, the Hellenic Statistical Authority's STEP-92 Statistical Classification of Occupations. The precise calculation of the index is as follows:

1. Generate a measure of prorated monthly income, using the data on earnings and calendar days employed. For employment spells lasting less than the entire month, the earnings were prorated (scaled up) to the equivalent an individual would have earned had they been employed for the full calendar month.
2. Keeping only individuals on regular, open-ended employment contracts and excluding individuals with earnings below the statutory minimum wage (applied on a monthly level) and excluding outliers with extremely high wages (excluding the top one percent of the distribution).
3. Convert the nominal prorated monthly earnings into real values, taking HICP inflation for Greece and using a base year of 2015 (Eurostat, 2023[16]).

The procedure was repeated using occupation based on several levels of aggregation of the occupational codes. While the analysis in OECD (2024[1]) impact evaluation uses six-digit ISCO codes, the high correlation between indices calculated based on the two codes (with a correlation coefficient of 0.82) indicates that the choice of which index is applied should not materially affect the results. Figure 4.1 plots the relationship between the two. Both codes exhibit a greater dispersion in the indices towards the upper end of the distribution, reflecting the fact that specialist occupations with high wages are not aggregated with lower-paying ones to the same degree.

Figure 4.1. Occupational indices based on six-digit or four-digit codes are similar

Occupational indices calculated at different levels of disaggregation

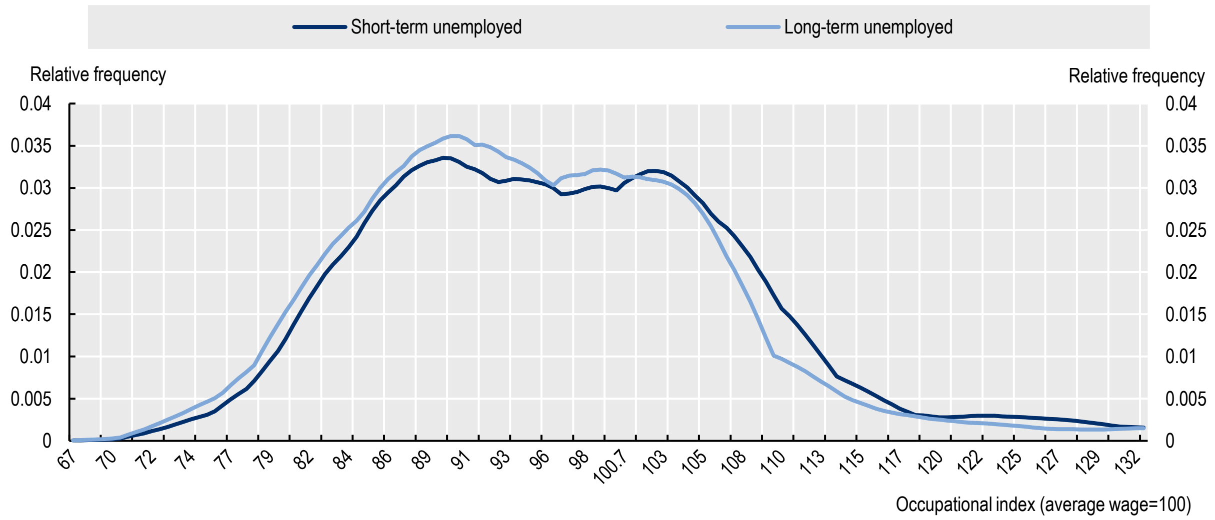


Note: Each shaded circle represents a six-digit occupational code based on Greece's national classification, the Hellenic Statistical Authority's STEP-92 Statistical Classification of Occupation. For each occupation, the index is calculated to the average real wage observed in the ERGANI employment data, which is assigned an index value of 100 by construction. The size of shaded circles is proportional to the number of individuals observed to be employed in the six-digit occupation during the 2017-21 period. Index values greater than 180 are not displayed.
Source: OECD calculations based on data from the Greek public employment service (DYPA) and ERGANI.

The occupational index distribution for Greece highlights the fact that individuals experiencing long-term unemployment disproportionally come from lower-ranked occupations (Figure 4.2). Individuals experiencing longer periods of unemployment are disproportionally those whose previous occupations were ranked lower than those who are in short-term unemployment. On average, individuals who are long-term unemployed have an occupational index that is 1.7 percentage points lower.

Figure 4.2. Long-term unemployed tend to come from lower in the occupational distribution

Occupational index distribution for short-term and long-term unemployed in Greece



Note: The heights of the lines indicate the relative share of individuals in occupations whose average wages are on the horizontal axis, relative to the average real wage observed in the ERGANI employment data. The distributions are calculated for all individuals who were unemployed during the 2017-21 period. Observations with index values above 132 are excluded from the kernel density chart.
Source: OECD calculations based on data from the Greek public employment service (DYPA) and ERGANI.

4.4. Several factors needed to be considered in choosing the econometric technique

CIEs aim to assess the specific effects of a programme on its participants and to distinguish these effects from those caused by other external factors. For example, if individuals participating in an ALMP have better employment prospects, CIEs help to determine whether this improvement is directly attributable to the ALMP intervention, rather than being influenced by broader economic trends or changes in other policies.

The aim of CIEs is therefore to compare the outcomes of individuals who have benefited from a programme (the treatment group) with those of a set of individuals as similar as possible (the control group). The only difference between the treatment group and the control group is that the latter did not participate in the programme. The control group therefore provides information on "what would have happened to the individuals exposed to the intervention if they had not been exposed to it", i.e. the counterfactual.

Three types of approaches can be adopted in a CIE: experimental evaluations, also called randomised controlled trials (RCTs), quasi-experimental evaluations. In an RCT, participants and non-participants are randomly assigned to an ALMP and the outcomes of these two (or more) groups are measured. Randomising participation in the ALMP minimises the chance that there are systematic differences between participants and non-participants that are unrelated to participation in the programme. For example, if participants are somehow more motivated or capable than non-participants - which can happen when selection into a programme is voluntary rather than random - simply comparing participants and non-participants would lead to biased estimates of the treatment effects of the programme. RCTs are designed to eliminate such sources of bias.

Despite these clear advantages, RCTs are not always appropriate for evaluating ALMPs. In practice, it can be difficult to randomise participation (or even the timing of participation) in a programme: indeed, policymakers often seek to carefully target policies to those most in need of support (for example, those most at risk of long-term unemployment). In addition, randomising participation requires planning the evaluation in advance of the launch of an intervention, may require monitoring to ensure compliance with the assigned treatment or control group, and may require the retention of additional data to allow for possible non-compliance later in the analysis phase of the evaluation.

Given these factors, alternative methods - which also compare participants and non-participants to estimate policy impacts - are another tool for evaluating ALMPs. Unlike RCTs, quasi-experiments do not randomly assign people to participate in the policy under evaluation. Instead, other methods are used to ensure that comparisons between participants and non-participants provide reliable estimates of the treatment effects of policies. For example, it may be possible to observe - and thus control for - any differences between participants and non-participants that would bias estimates of programme effects: this is the central tenet of matching methods and multivariate regression. Similarly, it may be possible to assume that comparing the changes experienced by participants and non-participants provides a reliable estimate of programme impact, even if it is not possible to compare the levels of key outcome variables. This difference-in-differences approach essentially compares the 'value added' of a programme among participants and non-participants. Finally, it may be possible to use the specific eligibility criteria of programmes. If individuals are eligible for a programme only above or below a precise cut-off point with respect to some key variable (e.g. age or household income), then participants and non-participants on either side of such a cut-off point should be similar in all respects except their participation in the programme. Comparing such individuals – as is the aim of a regression discontinuity design – should therefore provide robust estimates of programme impacts (more discussion on applying the different evaluation methods to conduct CIEs of ALMPs can be found in OECD (2020[18])).

In the case of the wage subsidy programmes studied in Greece, a regression discontinuity design could in principle have been used to examine some of the programmes' features, but the small sample sizes made this infeasible in practice. For example, two of the larger wage subsidy programmes required employers to retain workers for three months after the subsidy was exhausted, but only for jobseekers who had been unemployed for less than 12 months before entering the programme. In principle, the effect of the retention requirement could be examined by comparing the effects on individuals immediately below and above the 12-month threshold. If a sufficient number of individuals were observed in such groups - and the data contained precise information on which group each individual belonged to - this could be used to examine differences in the programme parameters. In practice, however, only a few hundred participants were observed close to the threshold, and it was not unambiguously clear from the wage subsidy data whether the retention requirement applied or not: it is not clear what date is taken to determine which programme parameters apply. Future evaluations would therefore benefit from specific, individual-level information on the programme parameters applicable to individual participants, as well as a larger number of participants around the threshold, which can be achieved if DYPA referred to subsidies more people around the threshold.

In the training and wage subsidy programmes evaluated in the impact evaluation, participation was not randomly assigned and there were not any strict eligibility criteria that could be used in the evaluation. Participation in the programmes was the result of a multitude of factors, and specific programmes did have strict eligibility criteria. However, these criteria often collectively covered a wide range of jobseekers across the programmes, negating the possibility of using another identification approach. For example, the two ICT training programmes evaluated targeted, respectively, individuals between 25-29 and 30-45 years old.

Furthermore, three of the largest four wage subsidy programmes targeted individuals aged 18-29, 30-49, and over 50 (for details of the wage subsidy programmes' parameters, see Table B.2).

4.5. The econometric approach addressed several challenges in identifying the programmes' effects

In order to account for the differing composition of participant and non-participant jobseekers in the programmes examined, an econometric approach that matches individuals on observable characteristics is adopted. This approach, called propensity score matching, attempts to ensure the comparability of the treatment and control groups and provide reliable estimates of the effects of both training programmes. Specifically, a rich set of observable characteristics is used to identify individuals with similar probabilities of enrolling into these training programmes. Individuals are then paired with similar individuals based on this probability and their outcomes are compared. Such an approach – based on a so-called propensity score – is commonly used in the literature to address the difficulty of otherwise accounting for a wide array of additional personal characteristics (Card, Kluve and Weber, 2018[19]).

In this report, a selection-on-observables approach is implemented which first applies nearest neighbour propensity score matching with exact matching on a select group of characteristics. This entails the following:

1. Calculating propensity scores based on a rich set of covariates – each individual's employment history (earnings, occupation, duration of employment), unemployment duration, employability rating (rule- based profiling score augmented by DYPA counsellor judgement), demographic characteristics (education, gender, nationality), foreign language skills, and location. The scores are calculated separately for each combination of programme and calendar year.
2. Matching exactly on each pre-treatment employment history (denoted by y, the number of years in the preceding three years when an individual had any paid employment; $y \in \{0, \{1 \; or \; 2\}, 3\},$), calendar month and year of entry into the programme, gender, earnings (9 groups in total), as well as – for individuals without prior employment in the preceding three years – age group (under 30, 30-50, over 50) and education (two groups); in other words, grouping individuals with exactly the same values of characteristics.
3. Within the groups defined in the second step, conducting nearest neighbour matching – pairing individuals with similar propensity scores. This is done for individuals on a month-by-month basis, matching individuals with similar characteristics in that calendar month.
4. Estimating treatment effects separately for each time horizon of interest (t, the amount of time elapsed since the start of the ALMP measure, when the outcome variables such as employment and earnings are measured). Denoting *potential* labour market outcomes (such as employment or earnings) for an individual (i) as Y_{imt}^{d}, where $d = 1$ under treatment and $d = 0$ otherwise, the average treatment effect on the treated ($D_{im} = 1$) for each t is then:

$$\gamma_t = E[Y_{imt}^{1} | D_{im} = 1] - E[Y_{imt}^{0} | D_{im} = 1]$$

In the above equation, γ_t are the key treatment effects reported for labour market outcomes in this analysis, looking at individuals every 3 months from month 3 to 36 after entering treatment (for individuals in the treatment group) or after being matched to an individual in the treatment group (for individuals not entering treatment). Given the exact matching on calendar month and year and the nearest neighbour matching, any time-specific effects are differenced out by construction.

Several different estimators can be used to calculate the average treatment effect of the programmes through the propensity score. Note that the propensity score measures the probability of individuals being treated, given their covariates. The inverse probability weighting estimator measures treatment effects by weighting outcomes by the inverse of the individual's propensity score. Another commonly used estimator is the kernel matching estimator, which matches each treated individual to a weighted average of all controls, with weights that are inversely proportional to the difference between the propensity scores of the treated and controls. This has the advantage of using all available information, resulting in lower variance. However, the disadvantage is that the observations used may be poor matches.

The question of which estimator performs best in empirical applications having been examined in several studies, but without an unambiguous answer (Frölich, 2004[20]; Huber, Lechner and Wunsch, 2013[21]; Busso, DiNardo and McCrary, 2014[22]). The lack of a preferred estimator in the literature is arguably because the relative performance of estimators arguably depends strongly on features of the data-generating process, which is unknown to the empirical researcher in practice.

Given the large sample sizes in the analysis, with several thousand participants per year in each of the programmes analysed, the analysis uses nearest neighbour matching estimators. This has the advantage of having the lowest bias for all sample sizes, although nearest neighbour matching has the disadvantage of higher variance estimates (Huber, Lechner and Wunsch, 2013[21]). For large sample sizes, the superior bias properties become more important, as the absolute difference in precision relative to more efficient estimators decreases as the variances asymptotically approach zero.

4.6. The econometric approach resulted in control groups similar to the ALMP participants

A key identifying assumption in propensity score matching is that all outcome-relevant differences between programme participants and non-participants are captured in their observed characteristics. In other words, conditional on observed covariates, the selection into the treatment can be considered random (e.g. Imbens (2000[23])). The treatment effects obtained through propensity score matching can be interpreted as causal under the assumption that treatment and control groups are comparable conditional on observable characteristics. To examine the extent to which the matching procedure reduced differences between participants and non–participants, the standardised distance between treatment and controls is compared before and after matching across covariates (see Figure 4.3 and Figure 4.4). This balancing test shows that propensity score matching indeed improved the comparability of the treatment and control groups for both training and wage subsidy programmes.

The use of exact matching in conjunction with propensity score matching is motivated by the superior performance of this procedure on a variety of balancing tests. Recall that the procedure involves (coarsened) exact matching, using a combination of prior labour market history and demographic characteristics (as described in the previous section). Balancing tests without the exact matching step results in inferior matches, including for some arguably important characteristics such as previous employment duration (see Figure A.2 and Figure A.3). After the exact matching, the balancing tests generally result in improved matches. This is corroborated by the statistics on standardised bias summarised in Table 4.1.

Although the balancing test can be carried out only on observable characteristics, there are several reasons why such unobserved characteristics arguably do not play an important role. *Firstly,* the balancing tests show a decrease in the differences of almost all the 27 variables plotted in Figure 4.3 and Figure 4.4. Crucially, this includes also variables that were not included in the matching function due to questions about their reliability (these variables are self-reported by jobseekers and relate to their personal characteristics, e.g. having children – see discussion in Section 2.3.2). The fact that the matching algorithm managed to decrease the difference also for these characteristics suggests that it is likely also implicitly accounting for many unobserved characteristics. *Secondly,* the extensive set of covariates used in this study include exhaustive information on jobseekers in almost all aspects that have been shown by the literature to be predictive of unemployment outcomes. In particular, labour market histories and earnings capture much of the information contained in usually unobserved variables (Heckman et al., 1998[24]; Caliendo, Mahlstedt and Mitnik, 2017[25]). *Finally*, as will be discussed in the next chapter, statistical tests comparing the outcomes of treated and control groups *before* the treated entered an ALMP are consistent with this assumption: they show statistically insignificant results for the vast majority of periods.

Table 4.1. Using exact matching in conjunction with propensity score matching improved the comparability of the treatment and control groups

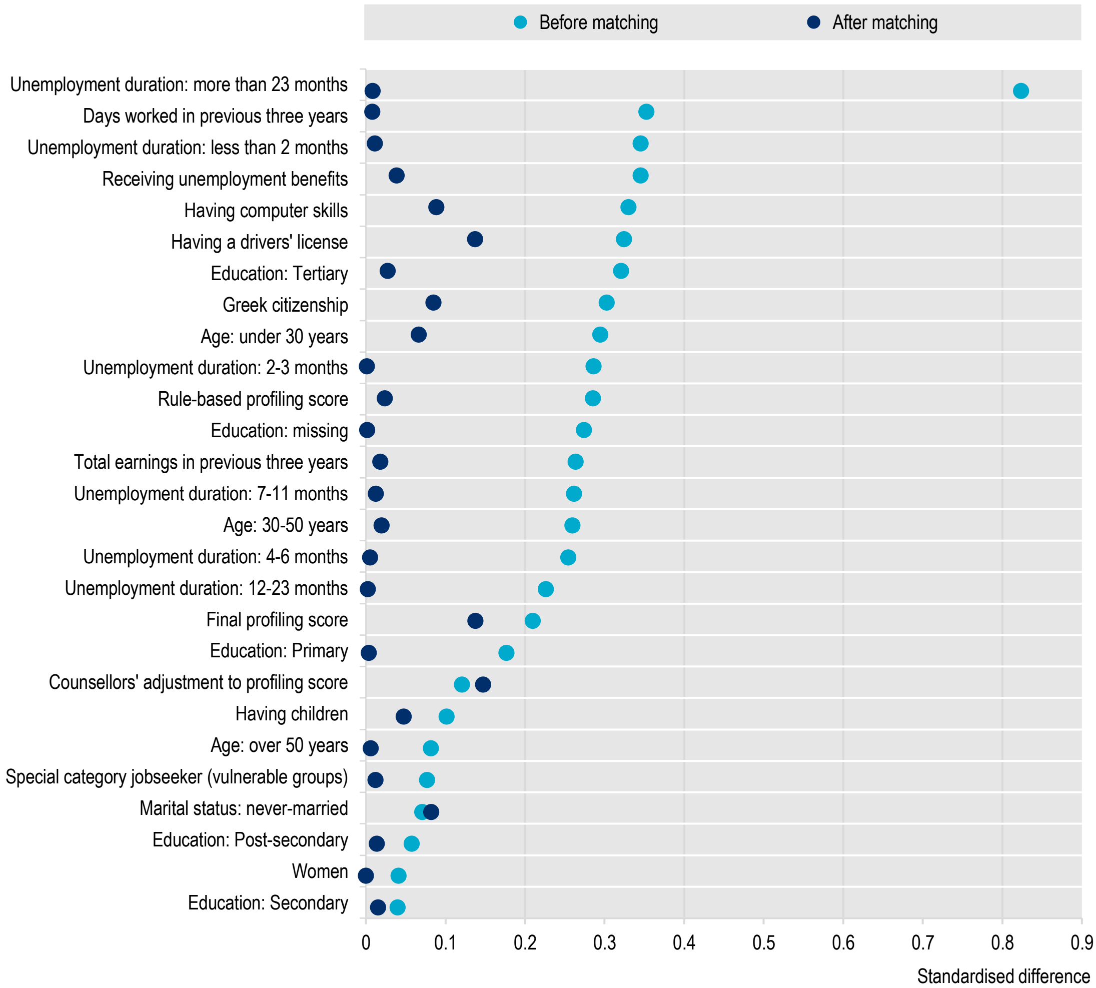
Mean and median standardised bias,[1] participants and other jobseekers, Greece

	Wage subsidies		Training	
	Mean	Median	Mean	Median
Unmatched observations (for reference)	30.5	31.8	36	37
Matched observations:				
- Combining propensity score matching with exact matching[2]	4.5	2.1	4.5	1.6
- Using only propensity score matching[3]	5	2	5.3	3.1

Note:
1. Standardised bias is the difference between the sample means in the treated and non-treated (full or matched) sub-samples as a percentage of the square root of the average of the sample variances in the treated and non-treated groups (Rosenbaum and Rubin, 1985[26]).
2. Matching exactly on each pre-treatment employment history (denoted by y, the number of years in the preceding three years when an individual had any paid employment; y ∈{0 ,{1 or 2},3},), calendar month and year of entry into the programme, gender, earnings (9 groups in total), as well as – for individuals without prior employment in the preceding three years – age group (under 30, 30-50, over 50) and education (two groups). Propensity score matches are conducted for individuals within specific points in time (e.g., matching individuals with similar characteristics in April 2020).
3. Propensity score matches are still conducted within specific points in time.
Source: OECD calculations based on data from the Greek public employment service (DYPA), Diofantos and ERGANI.

Figure 4.3. Propensity score matching improved the comparability of the treatment and control groups for training programmes

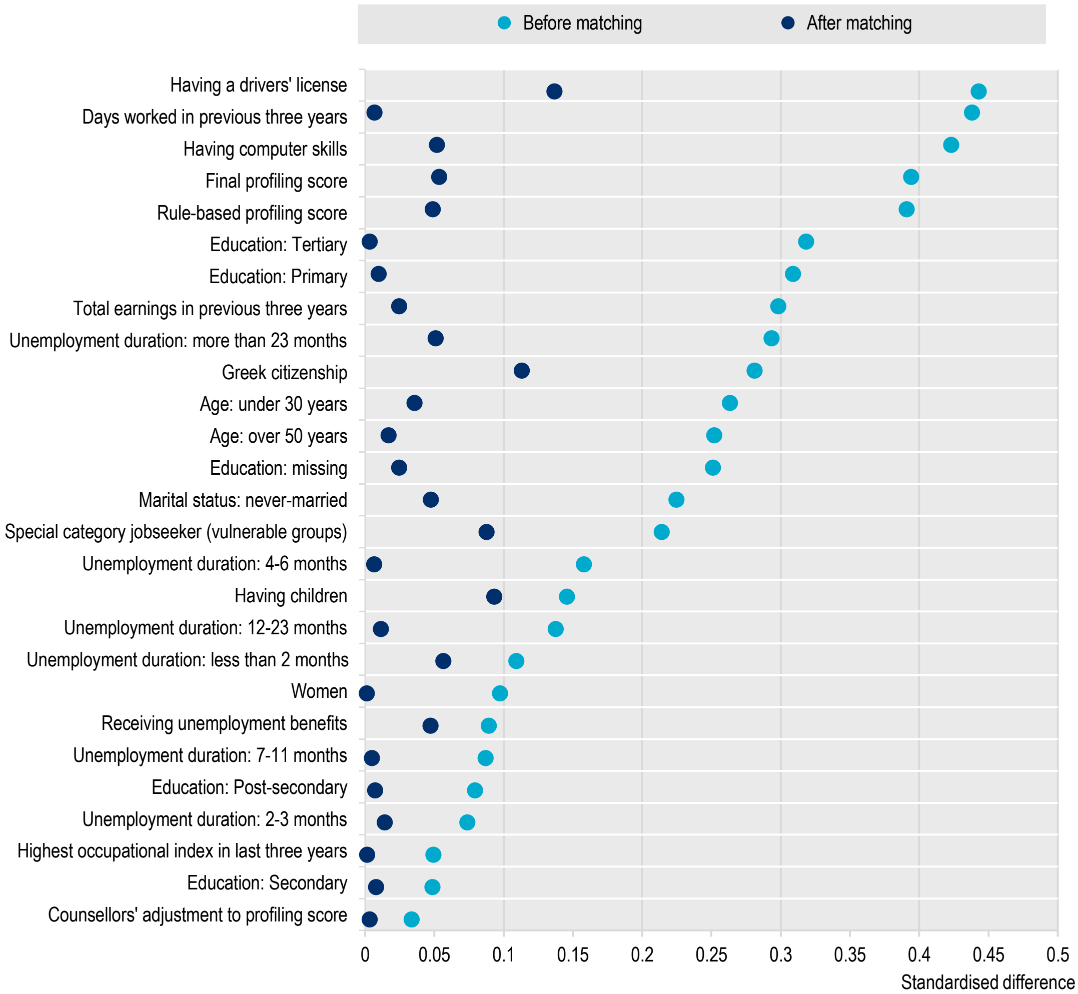
Standardised distance between treatment and control groups (0=no difference), training



Note: Figure presents standardised differences for the variables used in the propensity score matching, sorted descending from those with the greatest standardised differences before matching. Standardised differences are calculated as the difference in means between the treatment and control groups for the matching variable divided by the square root of the sums of the variances for that variable. Matching is conducted exactly on each pre-treatment employment history (denoted by m, the number of years in the preceding three years when an individual had any paid employment; m ∈{0 ,{1 or 2},3},), calendar month and year of entry into the programme, gender, earnings (9 groups in total), as well as – for individuals without prior employment in the preceding three years – age group (under 30, 30-50, over 50) and education (two groups).
Source: OECD calculations based on data from the Greek public employment service (DYPA), Diofantos and ERGANI.

Figure 4.4. Propensity score matching improved the comparability of the treatment and control groups for wage subsidy programmes

Standardised distance between treatment and control groups (0=no difference), wage subsidies



Note: Figure presents standardised differences for the variables used in the propensity score matching, sorted descending from those with the greatest standardised differences before matching. Standardised differences are calculated as the difference in means between the treatment and control groups for the matching variable divided by the square root of the sums of the variances for that variable. Matching is conducted exactly on each pre-treatment employment history (denoted by m, the number of years in the preceding three years when an individual had any paid employment; m ∈{0 ,{1 or 2},3},), calendar month and year of entry into the programme, gender, earnings (9 groups in total), as well as – for individuals without prior employment in the preceding three years – age group (under 30, 30-50, over 50) and education (two groups).
Source: OECD calculations based on data from the Greek public employment service (DYPA), Diofantos and ERGANI.

5 Additional results and robustness checks

In addition to the main results presented in the OECD (2024[1]) impact evaluation, a number of additional results have been generated during the course of the work on the project. These include a comparison of the pre-treatment outcomes between the treatment and control groups, results on the trajectories of the outcomes of treatment and control groups separately, additional results by groups of jobseekers or types of programmes, as well as additional results to determine whether the estimated results are sensitive to the particular econometric specification that has been used. These are presented in turn in this final chapter.

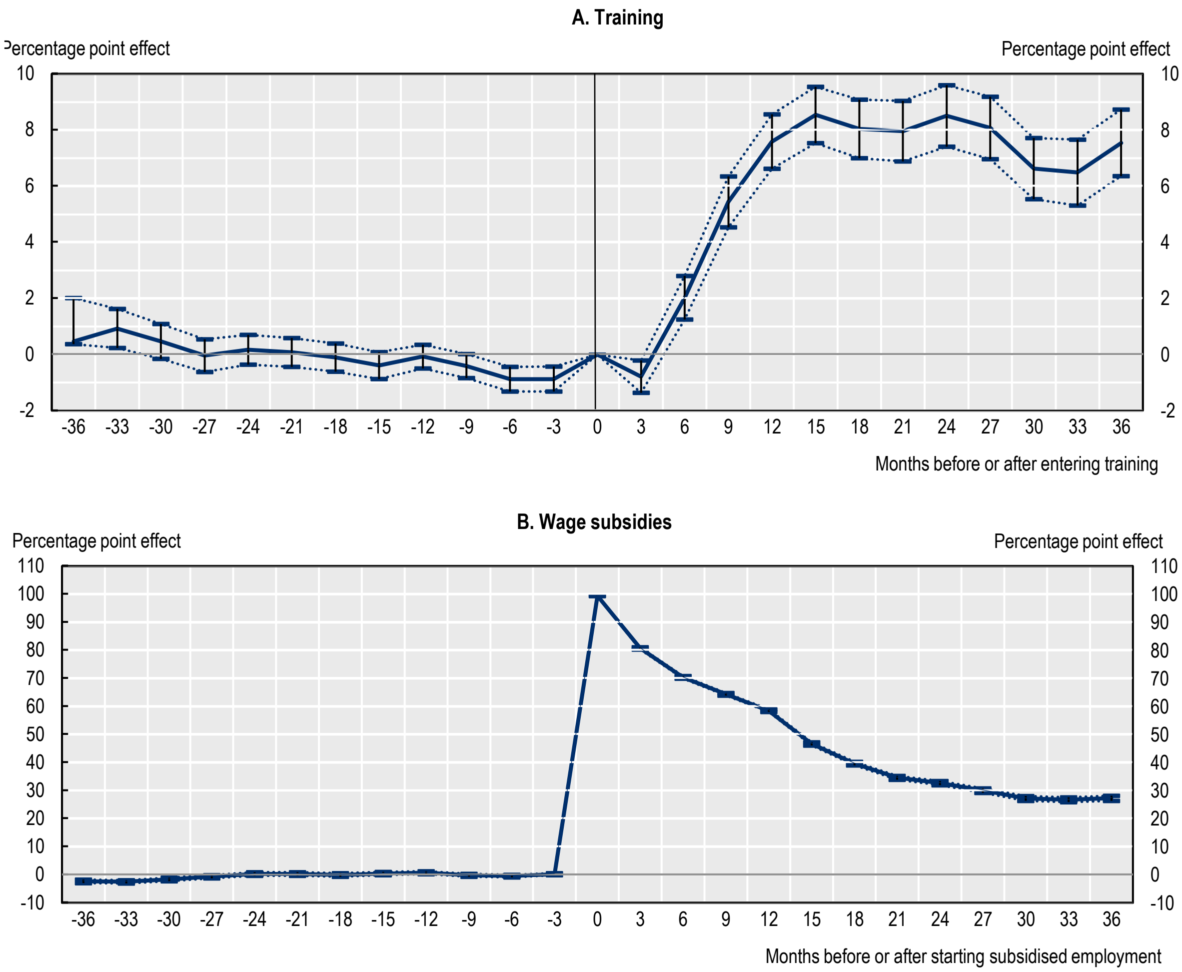
5.1. Differences in pre-treatment outcomes between participants and matched controls are mostly statistically insignificant

In addition to the balancing tests discussed in the previous section, an additional way to corroborate the suitability of the matching algorithm is to compare the pre-treatment outcomes of participants and their matched controls. This approach helps to ascertain whether the matching process has successfully created a control group that mirrors the treatment group prior to the intervention. If the pre-treatment outcomes are similar, this suggests that the matching has been effective in controlling for confounding variables that could otherwise bias the treatment effect. This step is important for affirming that any observed differences in outcomes post-treatment can be attributed to the treatment itself, rather than pre-existing disparities between the groups.

Differences between the pre-treatment employment outcomes for the participants and their matched controls are generally insignificant (Figure 5.1). Prior to entering treatment, the confidence intervals for both programmes studied generally include zero, although the estimates for some periods examined are marginally statistically significant. Qualitatively similar results are also found when comparing earnings (Figure A.4). After entering treatment, in contrast, the estimates indicate a significant divergence in outcomes after the treatment (see Chapter 4 and 5 for additional discussion). This divergence post-treatment corroborates the interpretation that the differences in outcomes can be interpreted as causal.

Figure 5.1. Pre-treatment employment outcomes corroborate similarity of treatment and control groups

Percentage point effect in employment probability



Note: The figure presents nearest-neighbour propensity score matching results. See Section 4.5 for details.
Source: OECD calculations based on data from the Greek public employment service (DYPA), Diofantos and ERGANI.

5.2. Additional results

This section presents additional results that complement the ones presented in the main impact evaluation report. This includes a description of selected labour market outcomes for matched control group individuals, additional outcome results for sub-groups of participants, and detailed results by specific programmes within the broader ALMPs.

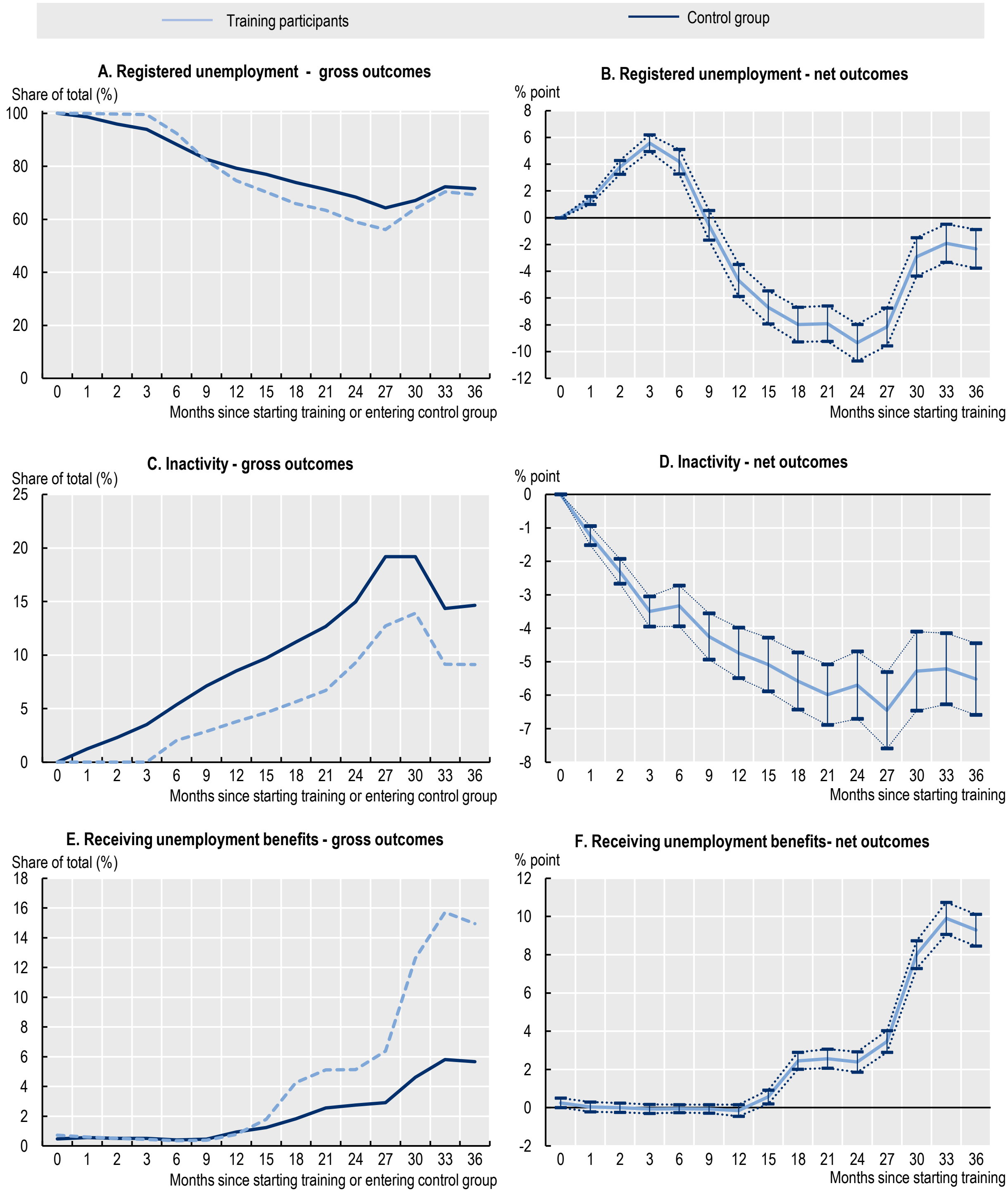
5.2.1. Additional results for training

As discussed in Chapter 4 of the main impact evaluation report, the training programmes examined in Greece improve the probability of finding employment, boost wages, extend the duration of employment, and increase overall earnings. Simultaneously, they reduce the incidence of registered unemployment and inactivity. With the exception of the initial training period, the impact of these programmes is positive across the three-year outcome horizons analysed. This section builds on the analysis in the main impact evaluation report by discussing some additional results.

Examining the outcomes of the training participants and the matched control group sheds light on the underlying dynamics of the treatment effects (Figure 5.2). The trajectories of the control group in registered unemployment (Panel A) clarify that the spike in the registered unemployment attributed to training (Panel B) is attributable to the continued unemployment of training participants while they are participating in the training. Virtually all the training participants remain registered as unemployed for the first four months of their training (including month zero, when they enter training). This roughly coincides with the duration of training participation. A similar pattern is observed in terms of exits into inactivity (Panels C and D). The pattern of the effects on unemployment benefit receipt is largely driven by the pattern of individuals returning into unemployment beginning around month 27 after entering the training programme. This can be explained by the renewed unemployment of some individuals who became employed after completing the training programme, with the employment period resetting their entitlement to unemployment benefits.

Figure 5.2. The labour market outcomes of the control group help clarify the estimated treatment effects of training

Percentage of individuals in unemployment, inactivity or receiving unemployment benefits and percentage point effects on these outcomes

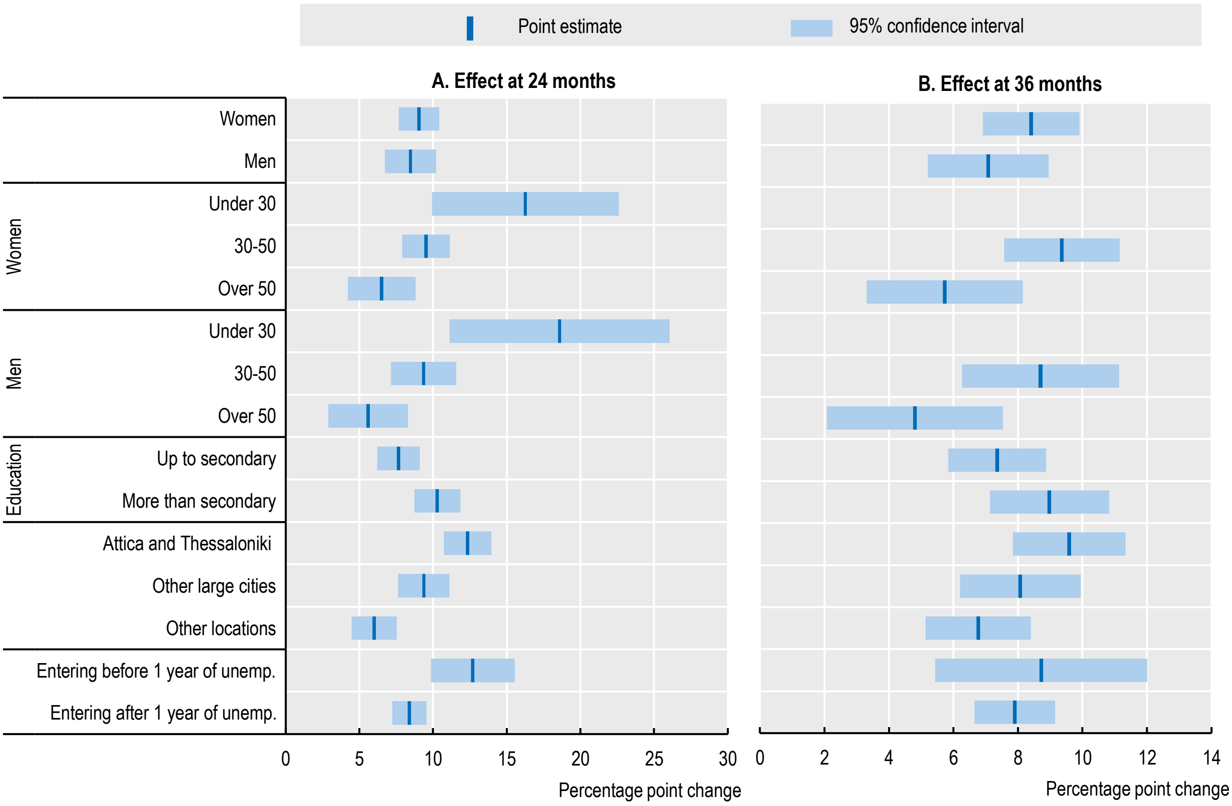


Note: The figure presents nearest-neighbour propensity score matching results. See Section 4.5 for details.
Source: OECD calculations based on data from the Greek public employment service (DYPA), Diofantos and ERGANI.

In terms of the effects of training on employment at different horizons across groups of workers, the effects are qualitatively similar at the longer time horizon of 36 months as they are at 24 months (Figure 5.3). The point estimates of the programmes' effects are still larger for younger workers, those with tertiary education, individuals in larger cities, as well as those unemployed for less than 12 months. However, due to smaller sample sizes at the longer intervals, the confidence intervals for the estimates are considerably larger. Note that the estimates at 36 months in fact reflect the estimates only for one of the training programmes – the training for high-demand sectors. This is due to a lack of data for the ICT training programmes, which commenced in early 2020 (the data on employment are only available through August 2022). The high-demand sectors training programme included only participants aged 29 and over, resulting in a lack of estimates for the youngest age group at 36 months (Panel B).

Figure 5.3. The effects of training across sub-groups are qualitatively similar at 24 and 36 months

Percentage point effect on employment probability



Note: The analysis presents nearest-neighbour propensity score matching results which matches individuals based on several characteristics: each individual's employment history (earnings, occupation, duration of employment), unemployment duration, employability rating (rule-based profiling score augmented by DYPA counsellor judgement), demographic characteristics (education, gender, nationality), foreign language skills, and location. For every individual in the treatment group, the matching is conducted based on the values of these characteristics in the calendar month when the individual enters the programme. The matched comparison group is comprised of individuals with similar characteristics not entering active labour market programmes in that same calendar month. For paired individuals in the treatment and control groups, this calendar month is then the reference point after which outcomes are measured. Some individuals in the treatment group are dropped as they do not have a corresponding match.
Source: OECD calculations based on data from the Greek public employment service (DYPA), Diofantos and ERGANI.

Mirroring their positive employment effects, all three training programmes are similarly effective at increasing cumulative earnings (Figure A.5) and cumulative days in employment (Figure A.6). The point estimates for all the subgroups examined are all positive, but due to the lower sample sizes, the estimates are less precisely estimated (with larger confidence intervals). This means that in some cases, the effects are not statistically significantly different from zero.

5.2.2. Additional results for wage subsidies

As discussed in Chapter 4 of the main impact evaluation report, the findings from the counterfactual impact evaluation indicate that the wage subsidy programmes examined in Greece have a positive effect on several labour market outcomes. Compared with the results of other studies of similar programmes in other countries, the estimated effects for Greece are generally much larger over all the time horizons examined (up to 36 months after initial entry into the programme). These large employment effects are observed without negative effects on occupational mobility in the longer term, although some slightly negative effects on occupational mobility are found for time horizons around 15 months. This section builds on the analysis in the main impact evaluation report by discussing some additional findings.

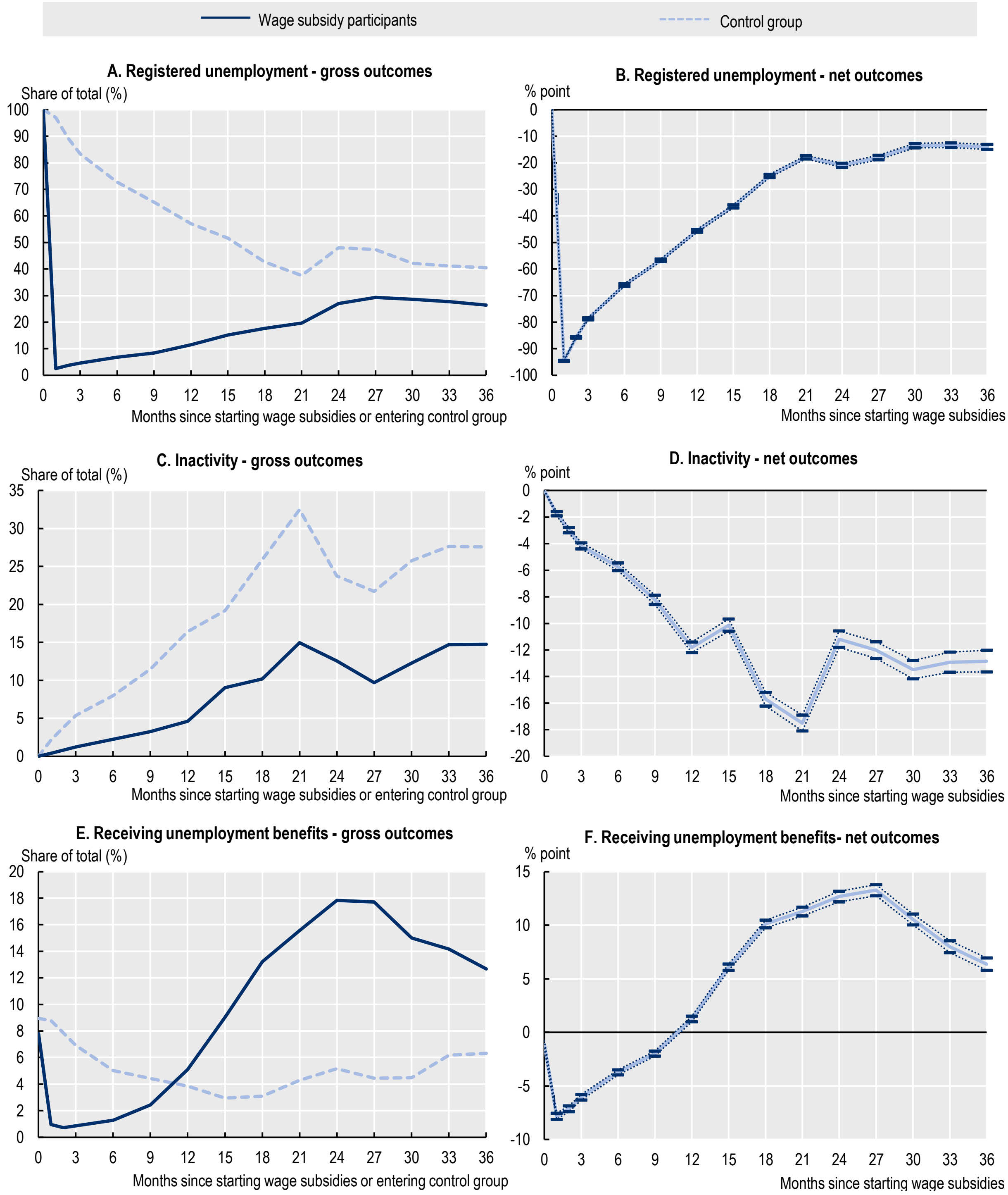
Examining the outcomes of the wage subsidy participants and the matched control group sheds light on the underlying dynamics of the treatment effects (Figure 5.4). While virtually all individuals in the treatment group exit registered unemployment by construction, the trajectory of the matched control group also shows a steady decline in registered unemployment (Panel A). This is partly attributable to exits into inactivity (Panel C), defined here as not being registered as unemployed or in formal employment. The pattern of the effects on unemployment benefit receipt is largely driven by the pattern of individuals returning into unemployment. The share of unemployment benefit recipients amongst wage subsidy participants begins to increase from around month 9 and (especially) month 12 after first entering a wage subsidy programme, coinciding with the duration of these programmes. This share peaks around months 24 to 27, after which it begins to decline. The latter can be explained by the duration for which individuals can receive unemployment benefits in Greece: this ranges from 5 to 12 months, with individuals qualifying for benefits based on their prior employment lasting at least 5 months (OECD, 2021[27]). An individual who became unemployed after 12 months of employment will have exhausted their unemployment benefits 24 months after they entered the wage subsidy programme.

5.2.3. Additional results by detailed geographic regions

A final interesting result concerns the results of the ALMPs on employment by detailed geographic region in Greece (Table B.3). In terms of the results on training, the long-term effects are largest in the two largest cities as well as Crete and Evia. They are smallest in the other islands. The profile of the effects over time also differed considerably across regions: on the islands (including Crete & Evia), the effects are larger earlier on in the observed period and moderating thereafter. In the other locations, the effects are either relatively flat (in the larger cities) or increasing (in all other locations). Similar to the training programmes, the wage subsidy programmes are found to be most effective in the two largest cities as well as Crete and Evia. In contrast to the training programmes, the magnitude of the effects decrease over longer time horizons. This likely relates mostly to the nature of the wage subsidy programmes, where all participants are employed at the beginning of the observation period by construction.

Figure 5.4. The labour market outcomes of the control group help better understand the estimated treatment effects of wage subsidies

Percentage of individuals in unemployment, inactivity or receiving unemployment benefits and percentage point effect on being in unemployment, inactivity or receiving unemployment benefits



Note: The analysis presents nearest-neighbour propensity score matching results. See Section 4.5 for details.
Source: OECD calculations based on data from the Greek public employment service (DYPA), Diofantos and ERGANI.

A relevant research question for designing wage subsidy programmes concerns how their effectiveness is affected by specific programme parameters, such as subsidy amount or duration. Unfortunately, the current analysis cannot sufficiently rigorously examine the effect of these specific parameters due to a lack of variation that could be credibly exploited to accurately isolate the programme effects. As shown in Table 5.1, during the period analysed, the programmes varied on some key features, but not in a way that is amenable to analysing the effects of a specific feature separately. Subsidies were typically offered for either nine or twelve months, with a minority extending to fifteen months and the possibility of further extensions in limited cases. The subsidy amount was commonly set at either 50% or 75% of the participant's wage. Notably, the higher subsidy rate was more prevalent in programmes initiated post-June 2020, a period that also saw other significant changes in programme implementation, including participant selection methods. This period also saw the abolishment of post-subsidy retention requirements. This collinearity in programme parameters makes it difficult to isolate a specific features effect in cross-programme comparisons.

Table 5.1. No clear relationship exists between wage subsidy programme parameters and estimated effects

Attributes and outcomes of selected wage subsidy programmes, sorted by treatment effect at 18 months

Programme attributes					Programme outcomes at 18 months	
Programme name (shortened)	**Subsidy duration (minimum)**	**Retention requirement at end of subsidy?**	**Wage subsidy amount (percent of wage)**	**First implemented after June 2020?**	**Treatment effect on employment (percentage points)**	**Treatment group employment rate (percent)**
Grant scheme for enterprises with up to 20 full-time jobs	9 months	At least 3 months	50	No	51.3	74.6
Programme of subsidies to enterprises for the recruitment of 8 300 unemployed persons aged 30 years and over	12 months	None	75	Yes	49.1	82.0
Programme for the employment of 6 000 unemployed persons aged up to 39 years	15 months	3 months	50	No	43.7	80.0
Enterprise grant scheme for the employment of 4 000 unemployed persons	12 months	Up to 3 months	50	No	43.2	70.5
Business support programme for the employment of 6 000 unemployed	12 months	Up to 3 months	50	No	42.0	77.4
Programme to support the first recruitment of young self-employed persons and young people	12 months	None	50	No	38.1	70.8
Programme for 10 000 socially and/or long-term unemployed people aged 30-49 years	9 months	3-6 months	50	No	38.1	67.5
Business grant scheme for the employment of 3 500 unemployed graduates of higher education institutions	10 months	None	75	Yes	24.6	64.7

Programme attributes					Programme outcomes at 18 months	
Programme name (shortened)	Subsidy duration (minimum)	Retention requirement at end of subsidy?	Wage subsidy amount (percent of wage)	First implemented after June 2020?	Treatment effect on employment (percentage points)	Treatment group employment rate (percent)
Programme of grants to enterprises for the employment of 8 000 unemployed young people aged between 18 and 29	12 months	None	75	Yes	23.8	59.2

Note: Includes wage subsidy programmes with at least 1 000 matched participants 18 months after programme entry. Given the time span of the data analysis, this means that a sufficient number of individuals had to have entered a particular programme between January 2017 and March 2021. Programme outcome columns report nearest-neighbour propensity score matching results that match individuals based on several characteristics: each individual's employment history (earnings, occupation, duration of employment), unemployment duration, employability rating (rule-based profiling score augmented by DYPA counsellor judgement), demographic characteristics (education, gender, nationality), foreign language skills, and location. For every individual in the treatment group, the matching is conducted based on the values of these characteristics in the calendar month when the individual enters the programme. The control group comprises individuals with similar characteristics not entering active labour market programmes in that same calendar month. For paired individuals in the treatment and control groups, this calendar month is then the reference point after which outcomes are measured. Some individuals in the treatment group are dropped as they do not have a corresponding match.
Source: OECD calculations based on data from the Greek public employment service (DYPA) and ERGANI.

5.3. Alternative estimation techniques and detailed results

This study employs a nearest neighbour direct matching estimator. It matches each treated individual with the nearest control in terms of propensity score. Given the large number of potential control group individuals in the Greek data - the entire population of registered unemployed - this technique allows to come as close as possible to an ideal exact matching scenario, where each treated individual would be matched with a control with exactly the same propensity score. In fact, this estimator has the smallest bias regardless of the sample size (Huber, Lechner and Wunsch, 2013[21]). Furthermore, the large sample size also allows reducing the variance, whose potential large size is the main drawback of this estimator.

Table B.4 show how the main results vary when a different matching estimator, one where the 10 nearest neighbours are used to calculate the counterfactual. The differences are extremely small, never amounting to more than half of a percentage point. Similar results are found when comparing the placebo estimates up to three years before the observation point: there, the differences never amount to more than 0.2 percentage points.

In order for the results of this evaluation to be as useful as possible also for future meta-analyses comparing the results across different studies, Table B.5 and Table B.6 provide the key estimates from the evaluation in tabular form. This includes the headline results as well as the results by specific subgroups. The table includes statistics commonly used in such meta-analyses, such as the number of participants and the standard errors of the estimates. These were also include in the two meta-analyses cited in the main report, by Card, Kluve and Weber (2018[19]) as well as the meta-analysis of projects funded by the EU's European Social Fund (European Commission and Ismeri Europa, 2023[28]).

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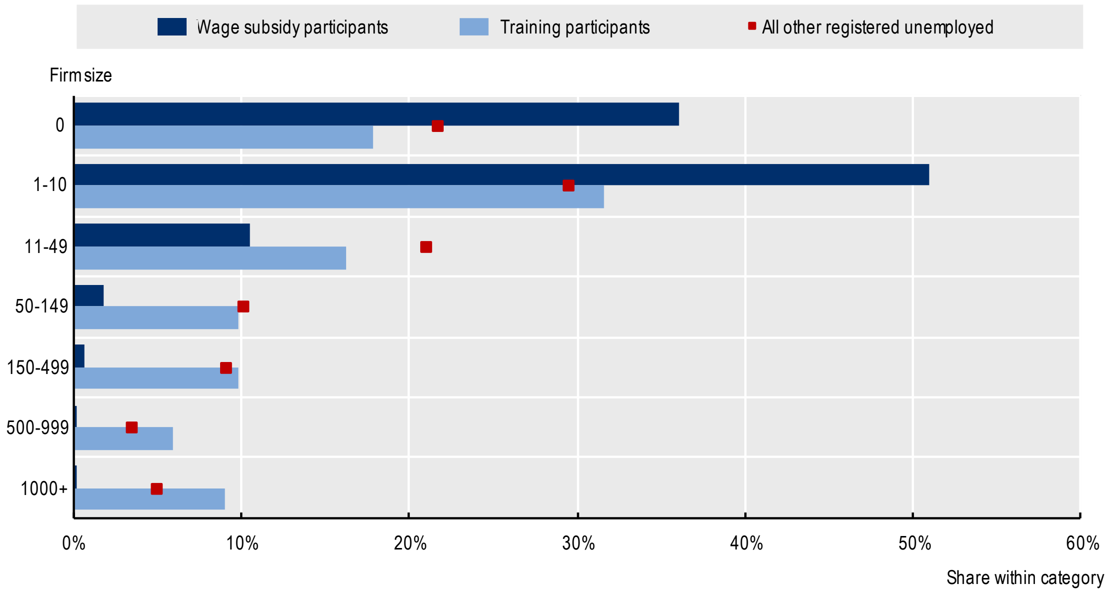
Annexe A. Additional figures

Figure A.1. Large employers did not commonly use wage subsidies even before state aid ceilings became more binding due to COVID-19 measures

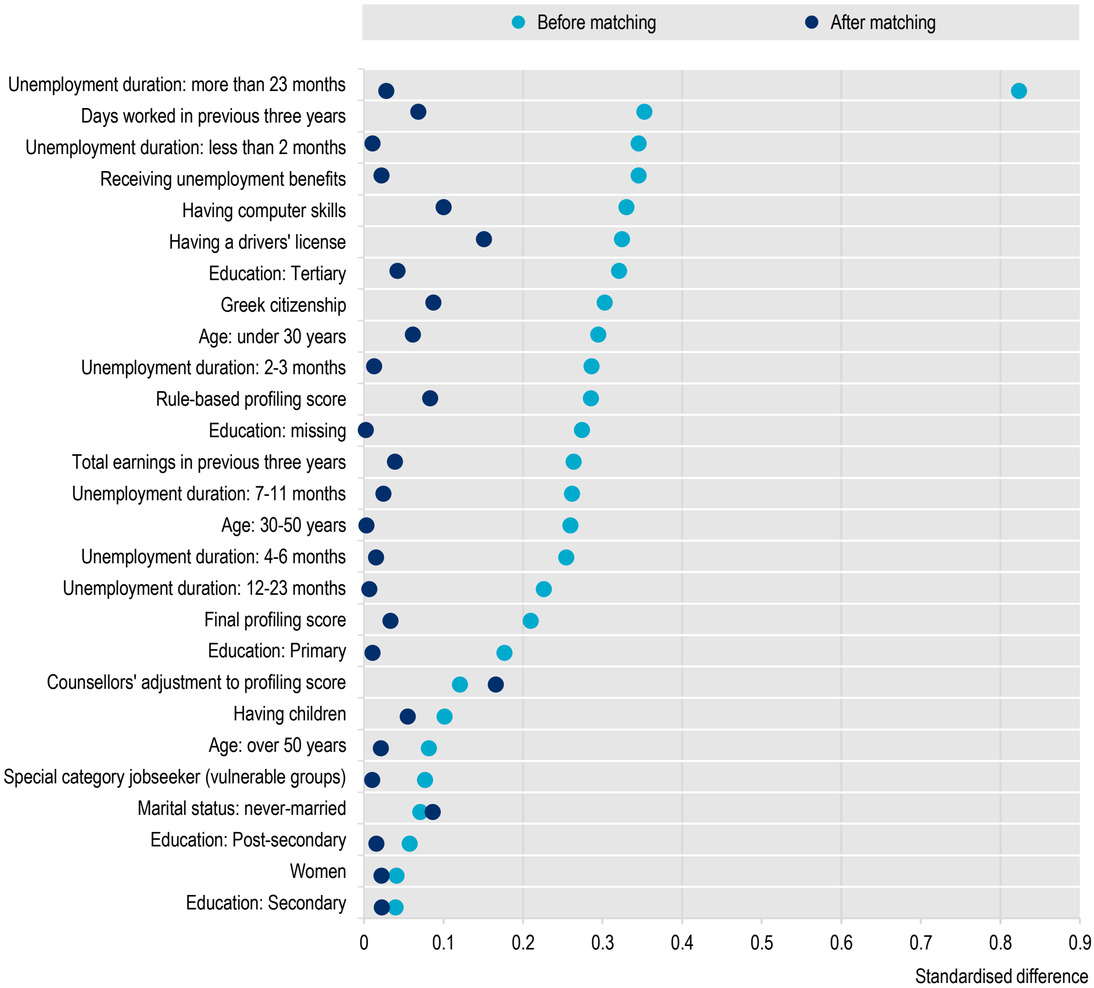
Share of ALMP participants and registered unemployed across firm size category, Greece, 2017-2020



Note: ALMPs stand for the training and wage subsidy programmes evaluated in this report. Shares are calculated within each of the five broad categories in the figure: if a demographic category of ALMP participants were represented in proportion to their share amongst all registered unemployed, the length of the bars would coincide with the red squares. Size categories are based on the total number of workers at a given employer registered in ERGANI on 1 January of the calendar year in which an individual became employed at that employer. Statistics for wage subsidy participants refer to the employers receiving the subsidies; statistics for the training participants refer to the first (unsubsidised) employer after completion of the theoretical and practical training. Statistics for stocks of all unemployed are calculated based on averages of monthly statistics during the 2017 21 period. Participant numbers refer to totals during the 2017 21 period for individuals entering either training or wage subsidies.
Source: OECD calculations based on data from the Greek public employment service (DYPA), Diofantos and ERGANI.

Figure A.2. Without exact matching, training participants would differ in important ways from their matched control group

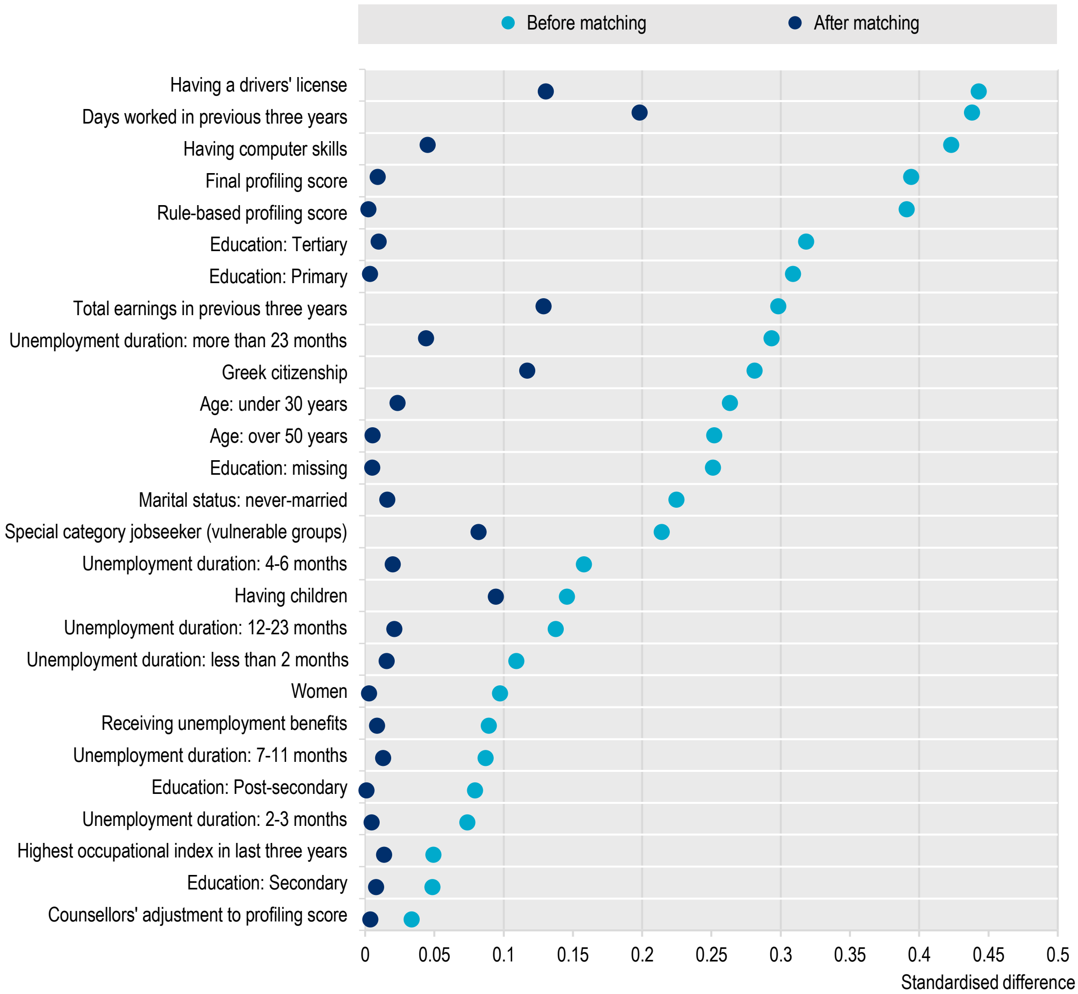
Standardised distance between treatment and control groups (0=no difference), training



Note: Figure presents standardised differences for the variables used in the propensity score matching, sorted descending from those with the greatest standardised differences before matching. Standardised differences are calculated as the difference in means between the treatment and control groups for the matching variable divided by the square root of the sums of the variances for that variable. Matching is conducted exactly on each pre-treatment employment history (denoted by m, the number of years in the preceding three years when an individual had any paid employment; m ∈{0 ,{1 or 2},3},), calendar month and year of entry into the programme, gender, earnings (9 groups in total), as well as – for individuals without prior employment in the preceding three years – age group (under 30, 30-50, over 50) and education (two groups).
Source: OECD calculations based on data from the Greek public employment service (DYPA), Diofantos and ERGANI.

Figure A.3. Without exact matching, wage subsidy participants also would differ in important ways from their matched control group

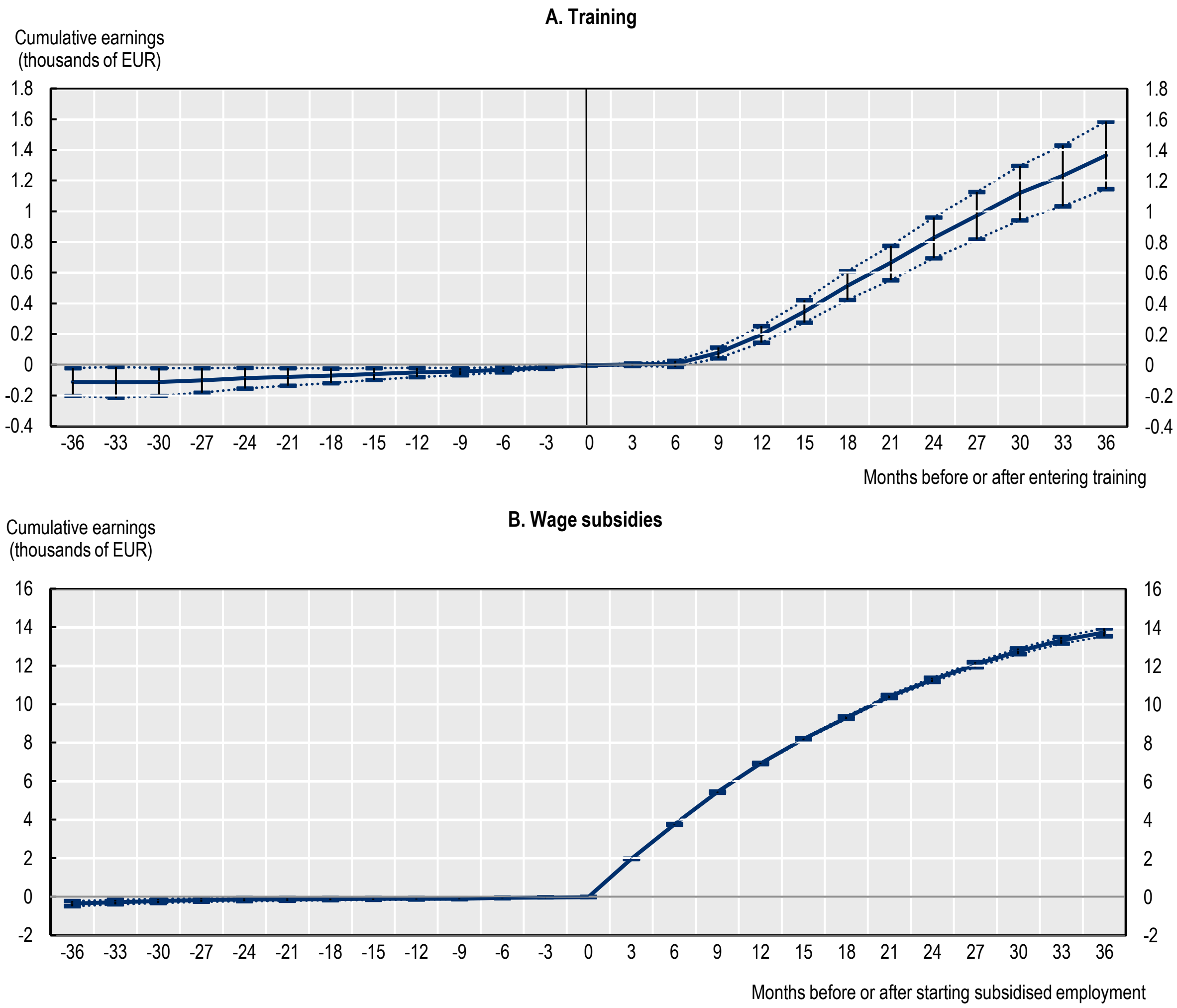
Standardised distance between treatment and control groups (0=no difference), wage subsidies



Note: Figure presents standardised differences for the variables used in the propensity score matching, sorted descending from those with the greatest standardised differences before matching. Standardised differences are calculated as the difference in means between the treatment and control groups for the matching variable divided by the square root of the sums of the variances for that variable. Matching is conducted exactly on each pre-treatment employment history (denoted by m, the number of years in the preceding three years when an individual had any paid employment; m ∈{0 ,{1 or 2},3},), calendar month and year of entry into the programme, gender, earnings (9 groups in total), as well as – for individuals without prior employment in the preceding three years – age group (under 30, 30-50, over 50) and education (two groups).
Source: OECD calculations based on data from the Greek public employment service (DYPA) and ERGANI.

Figure A.4. Pre-treatment earnings outcomes corroborate similarity of treatment and control groups

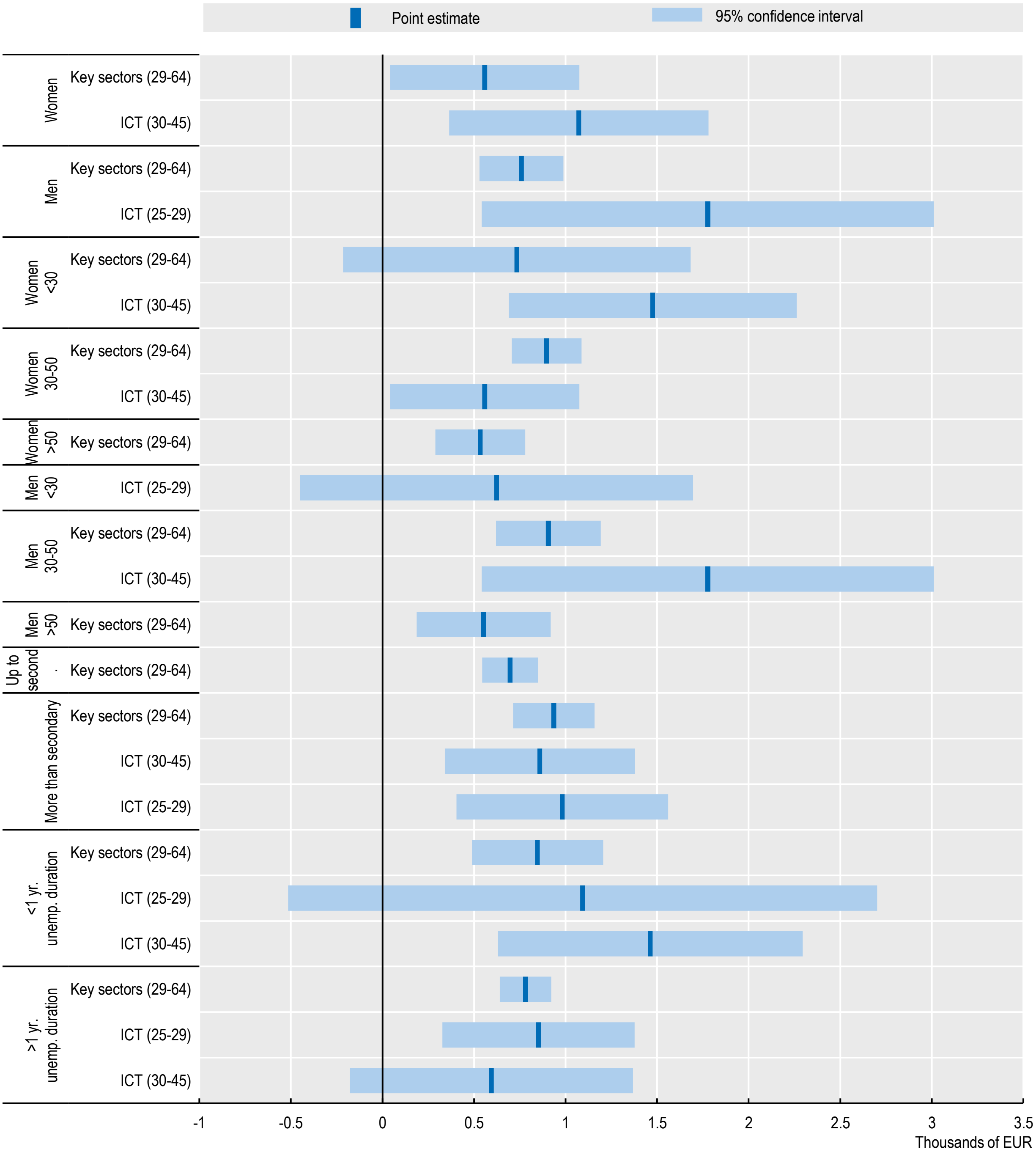
Effect on cumulative earnings



Note: The analysis presents nearest-neighbour propensity score matching results which matches individuals based on several characteristics: each individual's employment history (earnings, occupation, duration of employment), unemployment duration, employability rating (rule-based profiling score augmented by DYPA counsellor judgement), demographic characteristics (education, gender, nationality), foreign language skills, and location. For every individual in the treatment group, the matching is conducted based on the values of these characteristics in the calendar month when the individual enters the programme. The matched comparison group is comprised of individuals with similar characteristics not entering active labour market programmes in that same calendar month. For paired individuals in the treatment and control groups, this calendar month is then the reference point after which outcomes are measured. Some individuals in the treatment group are dropped as they do not have a corresponding match.
Source: OECD calculations based on data from the Greek public employment service (DYPA), Diofantos and ERGANI.

Figure A.5. The three training programmes increasing earnings for most groups

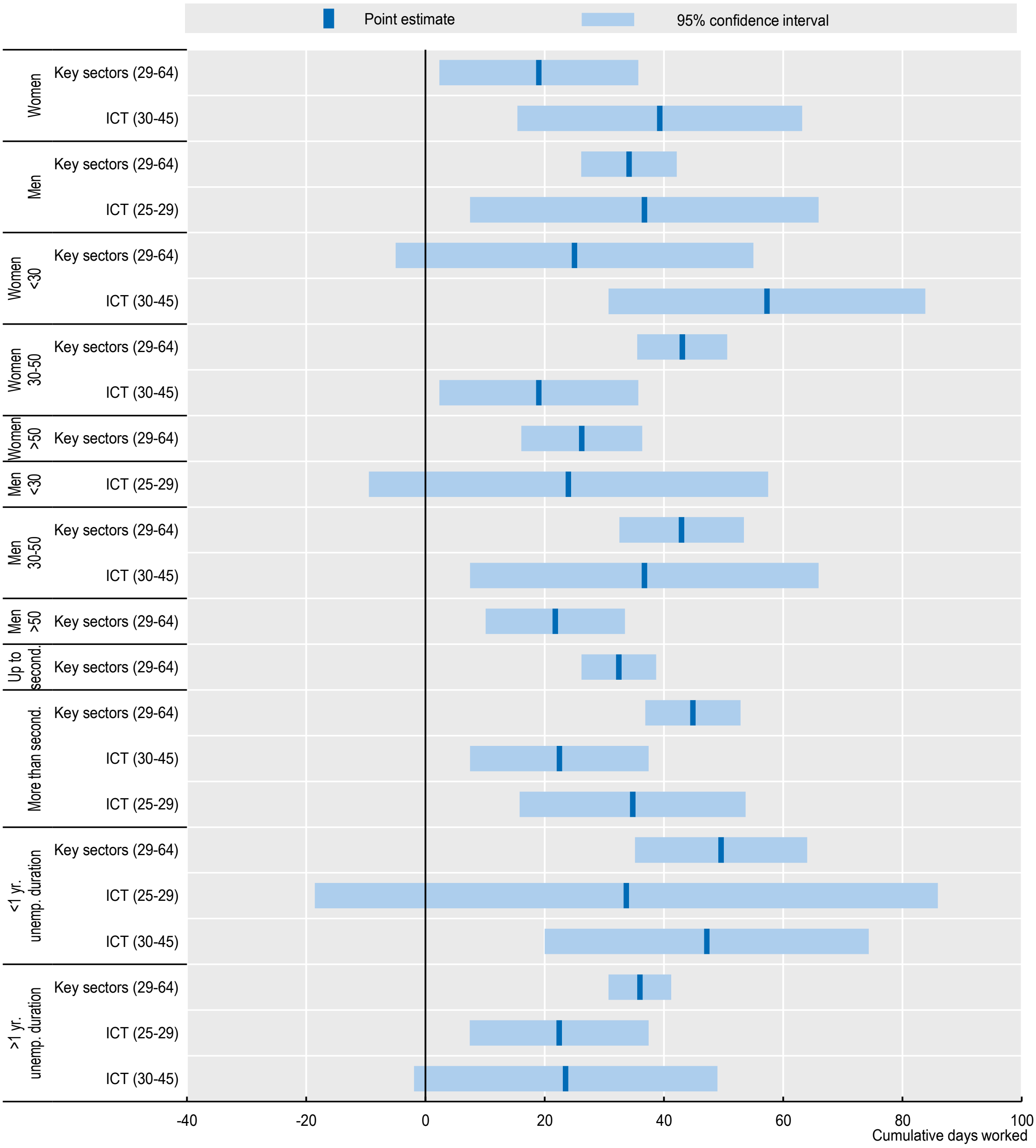
Effect on cumulative earnings at 24 months after starting training



Note: The analysis presents nearest-neighbour propensity score matching results which matches individuals based on several characteristics: each individual's employment history (earnings, occupation, duration of employment), unemployment duration, employability rating (rule-based profiling score augmented by DYPA counsellor judgement), demographic characteristics (education, gender, nationality), foreign language skills, and location. For every individual in the treatment group, the matching is conducted based on the values of these characteristics in the calendar month when the individual enters the programme. The control group is comprised of individuals with similar characteristics not entering active labour market programmes in that same calendar month. For paired individuals in the treatment and control groups, this calendar month is then the reference point after which outcomes are measured. Some individuals in the treatment group are dropped as they do not have a corresponding match.
Source: OECD calculations based on data from the Greek public employment service (DYPA), Diofantos and ERGANI.

Figure A.6. The three training programmes also resulted in increasing days worked for most groups

Effect on cumulative days worked 24 months after starting training



Note: The analysis presents nearest-neighbour propensity score matching results which matches individuals based on several characteristics: each individual's employment history (earnings, occupation, duration of employment), unemployment duration, employability rating (rule-based profiling score augmented by DYPA counsellor judgement), demographic characteristics (education, gender, nationality), foreign language skills, and location. For every individual in the treatment group, the matching is conducted based on the values of these characteristics in the calendar month when the individual enters the programme. The control group is comprised of individuals with similar characteristics not entering active labour market programmes in that same calendar month. For paired individuals in the treatment and control groups, this calendar month is then the reference point after which outcomes are measured. Some individuals in the treatment group are dropped as they do not have a corresponding match.

Source: OECD calculations based on data from the Greek public employment service (DYPA), Diofantos and ERGANI.

Annexe B. Additional tables

Table B.1. Data received in September 2023 filled-in gaps in employment information for roughly ten percent of jobseekers

Number of unique individuals in ERGANI data

	Count	Share of total
Individuals present only in data received September 2023	200 804	10.1%
Individuals present only in data received prior to September 2023	1 751423	88.1%
Individuals present in both datasets	5 977	0.3%
Total	1 986 755	100%

Source: OECD calculations based on data from ERGANI.

Table B.2. Many different wage subsidy programmes were examined

Key features of the wage subsidy programmes examined in the impact evaluation, sorted descending based on number of participants

Wage subsidy code	Programme name- English	Programme name- Greek	Programme description	Eligibility (in terms of UN duration)	Age and other eligibility criteria	No empl. reduction in…	Subsidy duration	Subsidy amount (% of wage)	Retention obligation
363222-122	Programme for 10.000 socially and/or long-term unemployed people aged 30-49 years, public call 15/2017	*Προγραμμα νθε 10.000 ανεργων κοινων ή/και μακροχρονιων, ηλικιας 30-49 ετων, δημοσια προσκληση 15/2017*	10k UN and LTU (30-49)*	3m & LTU (12m+)	30-49	past 3m	9m+9m; 12m+9m (LTU)	50%	6m for all staff (if hired UN), 3m (if hired LTU)
363610-16	Programme of subsidies to enterprises for the recruitment of 8 300 unemployed persons aged 30 years and over	*Προγραμμα επιχορηγησης επιχειρησεων για την προσληψη 8.300 ανεργων, ηλικιας 30 ετων και ανω*	8.3k UN (30+)*	1m	30+	past 3m	12m+9m ; 12m+12m (LTU)	75%	
363610-18	Programme of grants to enterprises for the employment of 8 000 unemployed young people aged between 18 and 29	*Προγραμμα επιχορηγησης επιχειρησεων για την απασχοληση 8.000 ανεργων νεων ηλικιας 18-29 ετων*	8k UN (18-29)*	1m & LTU	18-29	past 3m	12m; 15m	75%	
363222-115	Grant scheme for enterprises with up to 20 full-time jobs for the recruitment of 10 000 unemployed persons aged over 50	*Προγραμμα επιχορηγησης επιχειρησεων με προσωπικο εως 20 θεσεων πληρους απασχολησης για τη προσληψη 10000 ανεργων ηλικιας ανω των 50 ετων*	10k in employers with <20 employees (50+)*	3m & LTU	50+	past 3m	9m+9m; 12m+9m (LTU)	50%	3m for LTU (if extended) and other UN; 6m for other UN if extended duration
363222-125	Business support programme for the employment of 6 000 unemployed, graduates of sustainable education aged 18-29 years	*Προγραμμα επιχορηγησης επιχειρησεων για την απασχοληση 6.000 ανεργων,πτυχιουχων αει-ατει ηλικιας 18-29 ετων*	6k Uni/TEI graduates (18-29)*	3m& LTU & 1m for KEA	18-29 + Uni or TEI graduates	past 3m	12m (& 3m retention); 15m (LTU, KEA), no retention obligation	50%	3m for UN, zero months for LTU & KEA

Wage subsidy code	Programme name- English	Programme name- Greek	Programme description	Eligibility (in terms of UN duration)	Age and other eligibility criteria	No empl. reduction in…	Subsidy duration	Subsidy amount (% of wage)	Retention obligation
363222-126	Enterprise grant scheme for the employment of 4 000 unemployed persons, other education levels, aged 18-29 years	*Προγραμμα επιχορηγησης επιχειρησεων για την απασχοληση 4.000 ανεργων,λοιπων εκπ/κων βαθμιδων ηλικιας 18-29 ετων*	4k other edu level (18-29)	3m& LTU & 1m for KEA	18-29	past 3m	12m (& 3m retention); 15m (LTU, KEA), no retention obligation	50%	3m for UN, zero months for LTU & KEA
363610-17	Business grant scheme for the employment of 3 500 unemployed graduates of higher education institutions, aged 22 to 29	*Προγραμμα επιχορηγησης επιχειρησεων για την απασχοληση 3.500 ανεργων πτυχιουχων ανωτατων εκπαιδευτικων ιδρυματων, ηλικιας 22 εως 29 ετων*	3.5k higher edu (22-29)		22-29 Uni or TEI graduates	past 3m	10m	75%	
363610-20	Programme of grants to enterprises for the employment of 9 200 beneficiaries of the "Labour market reintegration cheque" in 2020'.	*Προγραμμα επιχορηγησης επιχειρησεων για την απασχοληση 9.200 δικαιουχων «επιταγης επανενταξης στην αγορα εργασιας» ετους 2020"*	9.2k of LM re-integration voucher*	UN with 50% of remaining UB, LTU with at least 1 day of UB	at least 2m of remaining UB for LTU, at least 50% of initial UB for others	past 3m	up tp 12m	80%	
363222-132	Programme of subsidies to enterprises for the employment of unemployed persons aged 30 years and over in the least developed regions of the country	*Προγραμμα επιχορηγησης επιχειρησεων για την απασχοληση ανεργων ηλικιας 30 ετων και ανω στις λιγοτερο αναπτυγμενες περιφερειες (λαπ) της χωρας*	UN in least dev regions (30+) phase 1	1m & LTU	30+		12m	50% (30-49 and <12, UN); 60% (50+), 65% (LTU); 75% (LTU and 50+)	
363222-128	Programme for the employment of 6 000 unemployed persons aged up to 39 years, graduates of tertiary education, in sectors of smart specialisation (ris3) and productive activity	*Προγραμμα επιχορηγησης επιχειρησεων για την απασχοληση 6.000 ανεργων ηλικιας εως 39 ετων, αποφοιτων τριτοβαθμιας εκπαιδευσης σε κλαδους εξυπνης εξειδικευσης (ris3) και παραγωγικης δραστηριοτητας*	6k TE graduates in smart spec. and productive activity (<39)*		<39 and Uni and TEI graduates	past 3m	15m	50%	3m
363222-134	Programme of grants to enterprises for the employment of unemployed persons aged 30 years and over in the least developed regions (laps) of the country cycle B	*Προγραμμα επιχορηγησης επιχειρησεων για την απασχοληση ανεργων ηλικιας 30 ετων και ανω στις λιγοτερο αναπτυγμενες περιφερειες (λαπ) της χωρας β κυκλος"*	UN in least dev regions (30+) phase 2	1m & LTU	30+	past 3m	12m	50% (30-49 and <12, UN); 60% (50+), 65% (LTU); 75% (LTU and	

Wage subsidy code	Programme name- English	Programme name- Greek	Programme description	Eligibility (in terms of UN duration)	Age and other eligibility criteria	No empl. reduction in…	Subsidy duration	Subsidy amount (% of wage)	Retention obligation
								50+)	
363222-124	Programme to support the first recruitment of young self-employed persons and young people aged up to 35 years	*Προγραμμα επιχορηγησης της πρωτης προσληψης μισθωτου-ων απο αυτοαπασχολουμενους νεους και επιχειρισεις νεων, ηλικιας εως 35 ετων*	Young employers (<35) (UN 18+)	3m	18+		12m; 18 (for <30)	50%	
363610-21	Programme of grants to enterprises for the recruitment of 1 000 disadvantaged and particularly disadvantaged persons	*Προγραμμα επιχορηγησης επιχειρησεων για την προσληψη 1.000 ατομων, που βρισκονται σε μειονεκτικη θεση και σε ιδιαιτερα μειονεκτικη θεση*	1k disadvantaged and very disadvantaged persons**	LTU and 24m+ (very vulnerable)	For those in vuln situation: 6m UN registration or 18-24 or primary school graduates or 50+ or living alone and are in charge of dependent members		12m; 12+12 (very vulnerable)	50%	
363222-121	'Programme of grants to enterprises and employers in general for the employment of 10 000 persons entitled to a labour market reintegration cheque	*Προγραμμα επιχορηγησης επιχειρησεων και γενικα εργοδοτων για την απασχοληση 10000 δικαιουχων επιταγης επανενταξης στην αγορα εργασιας*	10k of LM re-integration voucher (employers <=10 employees)		at least 2m of remaining UB for LTU, at least 50% of initial UB for others	past 3m	12m	" A)360€+8€	3m
363222-133	Programme of grants to enterprises for the employment of unemployed persons aged 30 years and over in the regions in transition (met), with a focus on women	*Προγραμμα επιχορηγησης επιχειρησεων για την απασχοληση ανεργων ηλικιας 30 ετων και ανω στις περιφερειες σε μεταβαση (μετ), με εμφαση στις γυναικες*	UN in transition regions (30+, emphasis on women)*	1m	30+ (and 50+ for older category)	past 3m	12m	75% for women. 50% for men <12m UN and <49; 60% for mem 50+; 65% for men LTU; 70% for men 50+ LTU	
363222-135	Programme of grants to enterprises for the employment of 3 400 unemployed persons, de-lignification	Προγραμμα επιχορηγησης επιχειρησεων για την απασχοληση 3.400 ανεργων, (ο1νθλ) απολιγνιτοποιησης	3.4k de-lignitisation	1m	Only Western Macedonia and Peloponnisos	past 3m	12m-18m	100 for former DEI, etc. For all other employers: 80% for30-49,	

Wage subsidy code	Programme name- English	Programme name- Greek	Programme description	Eligibility (in terms of UN duration)	Age and other eligibility criteria	No empl. reduction in…	Subsidy duration	Subsidy amount (% of wage)	Retention obligation
								90% for 50+; 90% for LTU, 100% for LTU and 50+ and 100% for women	

Notes: LTU: long-term unemployed; UN: unemployed; LM: labour market; KEA: guaranteed minimum income; TE: tertiary education; * Conditional on non-employment reduction in last 3 months; ** Conditional on net employment increase
Source: Greek public employment service (DYPA).

Table B.3. Results by detailed region show heterogeneity across regions

Effects on employment by detailed region, Greece

Category	Time period	Treatment effect	Control group outcome	95% Confidence Interval: Minimum	95% Confidence Interval: Maximum	Standard error	Number of observations: Treated	Number of observations: Controls
Panel A. Training programmes								
Thessaloniki & Attiki (Including Pireus)	12	9.3	10.4	7.8	10.7	0.7	6 606	4 846
Thessaloniki & Attiki (Including Pireus)	24	12.3	13.4	10.7	14.0	0.8	6 602	4 842
Thessaloniki & Attiki (Including Pireus)	36	9.6	14.5	7.8	11.3	0.9	5 902	4 168
Other Large Cities	12	6.1	11.0	4.5	7.8	0.8	4 945	3 701
Other Large Cities	24	8.0	15.1	6.1	9.8	0.9	4 938	3 694
Other Large Cities	36	7.3	13.8	5.4	9.3	1.0	4 222	3 024
Crete & Evia	12	12.1	11.7	9.3	14.9	1.4	1 589	1 398
Crete & Evia	24	13.0	16.8	9.9	16.2	1.6	1 587	1 396
Crete & Evia	36	10.5	15.8	7.2	13.7	1.7	1 421	1 233
Islands (Except Crete & Evia)	12	12.1	11.9	8.4	15.7	1.9	902	832
Islands (Except Crete & Evia)	24	10.8	17.1	6.8	14.7	2.0	902	832
Islands (Except Crete & Evia)	36	5.2	17.3	1.1	9.3	2.1	827	759
All Other Locations	12	4.7	11.2	3.2	6.1	0.7	8 059	5 675
All Other Locations	24	5.5	15.3	3.9	7.1	0.8	8 054	5 670
All Other Locations	36	6.9	13.9	5.2	8.5	0.9	7 224	4 873
Panel B. Wage subsidy programmes								
Thessaloniki & Attiki (Including Pireus)	12	57.4	28.8	56.5	58.3	0.5	15 481	14 958
Thessaloniki & Attiki (Including Pireus)	24	39.7	30.2	38.4	41.1	0.7	9 172	9 053
Thessaloniki & Attiki (Including Pireus)	36	32.6	33.1	30.9	34.3	0.9	5 849	5 787
Other Large Cities	12	58.8	27.0	57.8	59.8	0.5	12 686	12 354
Other Large Cities	24	28.5	28.7	26.9	30.0	0.8	7 647	7 549
Other Large Cities	36	24.3	31.5	22.4	26.2	1.0	4 861	4 807
Crete & Evia	12	58.5	28.1	56.5	60.4	1.0	3 211	3 183
Crete & Evia	24	36.1	29.9	33.2	39.0	1.5	1 990	1 980
Crete & Evia	36	31.0	30.9	27.4	34.6	1.8	1 354	1 348
Islands (Except Crete & Evia)	12	54.3	31.6	51.9	56.8	1.2	2 197	2 188
Islands (Except Crete & Evia)	24	31.3	30.2	27.4	35.1	2.0	1 167	1 164
Islands (Except Crete & Evia)	36	24.6	33.5	19.6	29.5	2.5	737	736
All Other Locations	12	60.7	26.0	59.9	61.5	0.4	19 157	18 487
All Other Locations	24	29.6	26.8	28.4	30.9	0.6	10 954	10 766
All Other Locations	36	24.5	30.7	22.9	26.1	0.8	7 006	6 893

Note: The analysis presents nearest-neighbour propensity score matching results which matches individuals based on several characteristics: each individual's employment history (earnings, occupation, duration of employment), unemployment duration, employability rating (rule-based profiling score augmented by DYPA counsellor judgement), demographic characteristics (education, gender, nationality), foreign language skills, and location. For every individual in the treatment group, the matching is conducted based on the values of these characteristics in the calendar month when the individual enters the programme. The control group is comprised of individuals with similar characteristics not entering active labour market programmes in that same calendar month. For paired individuals in the treatment and control groups, this calendar month is then the reference point after which outcomes are measured. Some individuals in the treatment group are dropped as they do not have a corresponding match.
Source: OECD calculations based on data from the Greek public employment service (DYPA), Diofantos and ERGANI.

Table B.4. Alternative estimation technique yields virtually identical results

Comparison of nearest-neighbour and 10-nearest-neighbors estimates, Greece

	Treatment effect (percentage points)		Sample size		
Time period (months)	Baseline results (nearest-neigbor)	Results with 10-nearest neigbors	Treated	Controls (nearest neighbour)	Controls (10 nearest-neigbours)
Panel A. Training					
3	-1.1	-0.7	22 102	14 019	132 050
6	1.9	2.0	22 102	14 019	132 050
9	5.2	5.7	22 102	14 019	132 050
12	7.2	7.8	22 102	14 019	132 050
15	8.0	8.5	22 101	14 018	132 045
18	7.7	8.2	22 101	14 018	132 045
21	7.9	8.2	22 101	14 018	132 045
24	8.9	9.3	22 084	14 001	131 900
27	8.6	8.7	21 863	13 787	129 807
30	7.3	7.5	21 002	12 983	122 241
33	7.3	7.1	19 598	11 717	110 155
36	8.2	7.7	19 598	11 717	110 155
Panel B. Wage subsidies					
3	80.8	80.9	54 715	50 016	266 800
6	70.5	70.8	54 715	50 016	266 800
9	64.5	64.8	54 715	50 016	266 800
12	58.7	59.1	54 715	50 016	266 800
15	47.0	47.2	51 814	47 346	258 722
18	39.8	40.2	47 961	43 986	248 719
21	34.7	35.2	43 614	40 319	238 151
24	32.8	33.2	31 080	29 857	207 338
27	30.1	30.3	27 286	26 261	188 790
30	27.2	27.4	25 810	24 836	181 268
33	26.6	26.9	23 157	22 300	168 161
36	27.3	27.4	19 878	19 200	152 065

Note: The analysis presents nearest-neighbour propensity score matching results which matches individuals based on several characteristics: each individual's employment history (earnings, occupation, duration of employment), unemployment duration, employability rating (rule-based profiling score augmented by DYPA counsellor judgement), demographic characteristics (education, gender, nationality), foreign language skills, and location. For every individual in the treatment group, the matching is conducted based on the values of these characteristics in the calendar month when the individual enters the programme. The control group is comprised of individuals with similar characteristics not entering active labour market programmes in that same calendar month. For paired individuals in the treatment and control groups, this calendar month is then the reference point after which outcomes are measured. Some individuals in the treatment group are dropped as they do not have a corresponding match.
Source: OECD calculations based on data from the Greek public employment service (DYPA), Diofantos and ERGANI.

Table B.5. Impact evaluation results on training to be used for future meta-analyses

Effects on employment for various groups of jobseekers at 12, 24 and 26 months, Greece

				95% Confidence Interval				
Category	Time period	Treatment effect	Control group outcome	Minimum	Maximum	Standard error	Treated	Controls
All participants	12	7.2	10.9	6.3	8.2	0.5	22 102	14 019
All participants	24	8.9	14.8	7.8	10.0	0.6	22 084	14 001
All participants	36	8.2	14.1	7.0	9.4	0.6	19 598	11 717
Men under 30	12	4.9	26.4	-2.1	11.9	3.6	386	330
Men under 30	24	18.6	31.4	11.1	26.0	3.8	382	326
Men under 30	36	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
Men aged 30-50	12	8.0	13.9	6.0	10.0	1.0	4 188	3 038
Men aged 30-50	24	9.4	18.3	7.1	11.6	1.1	4 188	3 038
Men aged 30-50	36	8.7	19.7	6.3	11.1	1.2	3 735	2 607
Women under 30	12	12.6	23.4	6.7	18.4	3.0	501	470
Women under 30	24	16.3	34.9	9.9	22.6	3.2	498	467
Women under 30	36	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
Men over 50	12	3.2	7.0	0.8	5.6	1.2	2 663	1 661
Men over 50	24	5.6	8.3	2.9	8.3	1.4	2 663	1 661
Men over 50	36	4.8	8.6	2.1	7.5	1.4	2 663	1 661
Women aged 30-50	12	8.5	11.5	7.1	10.0	0.7	10 888	6 729
Women aged 30-50	24	9.5	16.4	7.9	11.1	0.8	10 879	6 720
Women aged 30-50	36	9.4	16.6	7.6	11.2	0.9	9 728	5 659
Women over 50	12	4.5	5.5	2.4	6.6	1.1	3 472	1 826
Women over 50	24	6.5	6.2	4.2	8.8	1.2	3 472	1 826
Women over 50	36	5.7	6.8	3.3	8.1	1.2	3 472	1 826
Those entering ALMP before 1 yr of unemp	12	8.7	16.8	6.1	11.3	1.3	2 372	2 087
Those entering ALMP before 1 yr of unemp	24	12.7	21.1	9.9	15.5	1.4	2 354	2 069
Those entering ALMP before 1 yr of unemp	36	8.7	19.1	5.4	12.0	1.7	1 731	1 475
Those entering ALMP after 1 yr of unemp	12	7.0	10.3	6.0	8.0	0.5	19 730	11 973
Those entering ALMP after 1 yr of unemp	24	8.4	14.1	7.2	9.6	0.6	19 730	11 973
Those entering ALMP after 1 yr of unemp	36	7.9	13.9	6.7	9.2	0.6	17 867	10 276
Up to secondary	12	6.6	7.8	5.4	7.9	0.7	11 670	6 368
Up to secondary	24	7.7	10.9	6.2	9.1	0.7	11 669	6 367
Up to secondary	36	7.4	11.7	5.8	8.9	0.8	11 580	6 278
More than secondary	12	8.0	14.7	6.6	9.4	0.7	10 232	7 497
More than secondary	24	10.3	19.5	8.7	11.8	0.8	10 216	7 481
More than secondary	36	9.0	18.4	7.1	10.8	0.9	7 820	5 285

Note: The analysis presents nearest-neighbour propensity score matching results which matches individuals based on several characteristics: each individual's employment history (earnings, occupation, duration of employment), unemployment duration, employability rating (rule-based profiling score augmented by DYPA counsellor judgement), demographic characteristics (education, gender, nationality), foreign language skills, and location. For every individual in the treatment group, the matching is conducted based on the values of these characteristics in the calendar month when the individual enters the programme. The control group is comprised of individuals with similar characteristics not entering active labour market programmes in that same calendar month. For paired individuals in the treatment and control groups, this calendar month is then the reference point after which outcomes are measured. Some individuals in the treatment group are dropped as they do not have a corresponding match.
Source: OECD calculations based on data from the Greek public employment service (DYPA), Diofantos and ERGANI.

Table B.6. Impact evaluation results on wage subsidies to be used for future meta-analyses

Effects on employment for various groups of jobseekers at 12, 24 and 26 months, Greece

				95% Confidence Interval				
Category	Time period	Treatment effect	Control group outcome	Minimum	Maximum	Standard error	Treated	Controls
All participants	12	58.7	27.7	58.2	59.2	0.3	54 715	50 016
All participants	24	32.8	28.6	32.0	33.6	0.4	31 080	29 857
All participants	36	27.3	31.7	26.4	28.3	0.5	19 878	19 200
Men under 30	12	50.9	33.2	49.5	52.3	0.7	7 445	6 789
Men under 30	24	34.1	32.6	31.8	36.4	1.2	3 209	3 129
Men under 30	36	28.1	38.2	25.3	31.0	1.5	2 245	2 187
Men aged 30-50	12	57.3	31.0	56.1	58.4	0.6	10 069	9 606
Men aged 30-50	24	32.6	32.0	30.9	34.2	0.8	6 609	6 448
Men aged 30-50	36	31.1	35.3	29.0	33.2	1.1	4 082	4 011
Women under 30	12	51.9	31.3	50.7	53.1	0.6	11 085	10 026
Women under 30	24	32.5	32.8	30.6	34.4	1.0	5 027	4 841
Women under 30	36	26.3	37.1	23.9	28.7	1.2	3 315	3 183
Men over 50	12	66.0	22.2	64.3	67.6	0.8	3 943	3 898
Men over 50	24	40.3	24.0	37.9	42.6	1.2	2 867	2 843
Men over 50	36	27.2	23.2	24.3	30.2	1.5	1 872	1 856
Women aged 30-50	12	63.6	23.6	62.7	64.4	0.4	16 561	15 847
Women aged 30-50	24	29.4	26.7	28.1	30.7	0.7	10 702	10 432
Women aged 30-50	36	27.6	29.9	26.0	29.3	0.8	6 669	6 512
Women over 50	12	71.9	16.8	70.3	73.5	0.8	3 625	3 579
Women over 50	24	38.4	19.6	35.9	40.9	1.3	2 510	2 490
Women over 50	36	17.2	20.0	14.1	20.3	1.6	1 622	1 608
Those entering ALMP before 1 yr of unemp	12	52.9	32.2	52.2	53.6	0.4	30 556	28 093
Those entering ALMP before 1 yr of unemp	24	31.9	33.1	30.9	33.0	0.5	16 815	16 338
Those entering ALMP before 1 yr of unemp	36	26.0	36.7	24.7	27.4	0.7	10 508	10 265
Those entering ALMP after 1 yr of unemp	12	67.1	20.8	66.4	67.8	0.4	22 177	21 271
Those entering ALMP after 1 yr of unemp	24	33.9	23.2	32.8	35.0	0.6	14 116	13 696
Those entering ALMP after 1 yr of unemp	36	28.7	26.1	27.3	30.1	0.7	9 300	9 024
Up to secondary	12	61.5	25.1	60.8	62.2	0.3	27 212	26 109
Up to secondary	24	33.7	25.9	32.7	34.7	0.5	16 766	16 419
Up to secondary	36	26.3	29.4	25.0	27.6	0.6	11 002	10 780
More than secondary	12	55.9	30.2	55.1	56.6	0.4	24 604	22 867
More than secondary	24	31.6	32.0	30.4	32.7	0.6	13 564	13 140
More than secondary	36	28.4	35.0	26.9	29.8	0.8	8 433	8 188

Note: The analysis presents nearest-neighbour propensity score matching results which matches individuals based on several characteristics: each individual's employment history (earnings, occupation, duration of employment), unemployment duration, employability rating (rule-based profiling score augmented by DYPA counsellor judgement), demographic characteristics (education, gender, nationality), foreign language skills, and location. For every individual in the treatment group, the matching is conducted based on the values of these characteristics in the calendar month when the individual enters the programme. The control group is comprised of individuals with similar characteristics not entering active labour market programmes in that same calendar month. For paired individuals in the treatment and control groups, this calendar month is then the reference point after which outcomes are measured. Some individuals in the treatment group are dropped as they do not have a corresponding match.
Source: OECD calculations based on data from the Greek public employment service (DYPA) and ERGANI.