Evaluating at Scale Under Real-World Constraints
Lessons from the *Fondo Repubblica Digitale*

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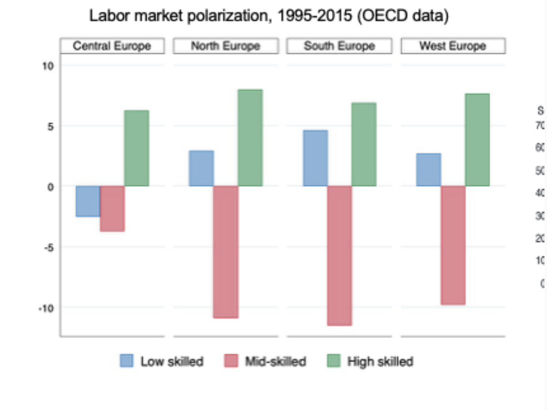
Motivation

The Global Context: Relentless Labor Market Polarization

Widespread transformations:

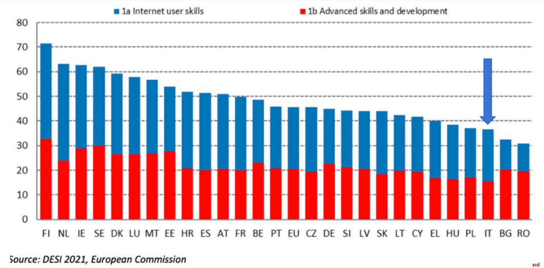
- De-industrialization across OECD countries
- Labor market polarization (middle-skill jobs declining)
- Growing skill mismatches among employed workers
- Accelerated by digital transformation



Source: OECD.

The Italian Context: A Severe Digital Skills Gap

- Italy ranks among lowest in EU for digital skills (DESI index)
- Large share of adults lack basic digital competencies
- Particularly acute among:
    - NEETs (15-34 years old)
    - Women outside labor force
    - Workers in at-risk sectors
- Limited access to quality digital training programs



Source: European Commission.

What We Know: Public Sector Policies

Active Labor Market Policies (ALMPs):

- Widely heterogeneous across countries
- Generally perceived as ineffective (Card et al., 2010, 2018)
- Average effects close to zero or small

But... promising innovations emerging:

- Sectoral employment programs (Year Up, Per Scholas, Project QUEST)
- J-PAL evidence (2022): Substantial earnings increases (10-25%)
- Common features: employer engagement, industry credentials, wrap-around support

The Opportunity: Fondo Repubblica Digitale

A unique policy initiative

- Large-scale public-private partnership with Italian banking foundations (EUR 350M over 5 years, 100,000 adults)
- Evaluation embedded *at design stage* (but no ex-ante randomization)
- Committed to scale based on evidence

My role (2022-2025)

- Advisor to Italian government on program design
- Chair of Independent Scientific Committee
- Led design and implementation of evaluation framework

Today's Objective: share lessons on doing rigorous evaluation under *real-world constraints*

Design Under Constraint

Key Features of the *Fondo Repubblica Digitale*

Innovative design elements:

- "Evaluability" as pre-condition for funding
  - Applicants must be willing to be evaluated with counterfactual methods
  - Commitment to standardized data collection
- Two-phase approach:
  - Years 1-2: Experimentation (multiple program models)
  - Years 3-5: Scale most effective programs
- Evaluation needed for scale-up selection
  - Evidence-based allocation of remaining funds
  - Programs with demonstrated impact prioritized

First Italian policy with such strong emphasis on evaluation

Building State Capacity: The Evaluation Lab

Independent Scientific Committee:

- Oriana Bandiera (LSE), Barbara Biasi (Yale), Francesco Drago (EUI), Luciano Floridi (Yale), Raffaella Sadun (HBS)

Evaluation Lab Team:

- 1 Hub Director
- Team of Research Assistants (10+ pre-docs)

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Key activities:

1. Design standard metrics and measurement approaches
2. Support evaluation design of proposals ex-ante
3. Evaluate individual programs ex-post
4. Build dashboard to compare effectiveness

Auxiliary goals: create lasting capability for the State, train new cohort of researchers, set standard for evaluation

First investments: ONLIFE and FUTURA

Allocation of EUR 13mn for a first rounds of experimental interventions to be scaled at a later stage

Target populations for the first round of investments

- **NEETs** (15-34 years): Not in employment, education, or training
- **Women** (18-50 years): whether Unemployed/inactive, or workers

Core idea

- Focus on *quality* programs leading to employment

Inspiration: Sectoral Employment Programs (J-PAL Evidence Review, 2022)

Program	Earnings Impact	Follow-up
Year Up	25% increase	6-9 years
Per Scholas	22% increase	6 years
Project QUEST	15% increase	9 years
WorkAdvance	10-15%	3 years

Shared features

1. Upfront screening for basic skills and motivation
2. Training targeted to high-wage sectors
3. Industry-recognized credentials
4. Career readiness training (soft skills)
5. Wrap-around support services
6. Strong employer connections

Methodology and Creative Solutions

Evaluation: Three constraints and their solutions

Objectives

- Assess and compare impact of 20+ experimental programs chosen by the administrative personnel of the *Fondo*
- Provide useful information for second phase (scale up)

Challenges

1. **Limited randomization** → External panel with interest-based screening
2. **Program heterogeneity** → Standardized measurement + heterogeneity analysis
3. **Implementation timing mismatches** → Exploding the panel

The Fundamental Econometric Problem

What we want to estimate

$$\text{ATE} = E[Y_i(1) - Y_i(0)]$$

where $Y_i(1)$ is outcome with training, $Y_i(0)$ is outcome without training

The problem

- We observe $Y_i(1)$ for treated individuals (program participants)
- We *never* observe $Y_i(0)$ for these same individuals
- We need a credible *counterfactual*: what would have happened without the program?

Why simple comparisons fail

- Participants self-select into programs (e.g. motivation, digital interest, job-seeking), implying that $E[Y_i(0)|D_i = 1] \neq E[Y_i(0)|D_i = 0]$)
- Implementers screen participants, leading to "cream skimming"

Constraint #1: The Randomization Barrier

The Gold Standard

- Randomized Controlled Trials (RCTs)

Issue: policymakers often won’t/can’t randomize or pay for oversubscription

- Political constraints: “Unfair to deny treatment”, time pressures
- Ethical concerns (real or perceived)
- Administrative complexity of randomization

The Research Challenge

- How do we produce rigorous causal evidence when we cannot control treatment assignment?

Our Approach: External Comparison Group

Core idea: Construct a comparison group that

1. Matches treated on observable characteristics
2. Did not have access to Fondo programs
3. Had similar alternative opportunities (other training, job search)

Method: External statistical panel

- Recruited 1,800 individuals matching target demographics
- Surveyed every 3 months (6 waves, Oct 2023 - Sep 2025)
- Same questionnaire as program participants
- Key innovation: Interest-based screening (next slide)

Identification assumption:

$$E[Y_i(0)|D_i = 1, X_i] = E[Y_i(0)|D_i = 0, X_i]$$

After conditioning on observables X_i, selection is as-good-as-random

Interest-Based Screening

The Comparability Problem

- Program participants are highly selected: motivated, interested in digital skills, job-seeking

Our solution: Screen panel on interest, not just demographics

1. Match target demographics (age, gender, region, education, employment status)
2. **Report high interest in free digital training**
3. **Report not having heard about such training opportunities**

Why this works (in theory, compliance is an issue)

- Mimics selection into program (interest + information)
- Panel members are *eligible but uninformed* comparators
- More credible counterfactual than random demographic match

Limitation: Still selection on observables, not unobservables

Constraint #2: Program Heterogeneity

Many interventions (by design)

- 23 different implementing organizations
- Each designed their own program within broad guidelines
- Varying training content, duration, intensity
- Different geographic coverage (local, regional, national)
- Different selection criteria for participants

The challenge

- How to evaluate 23 “different treatments” while ensuring comparability across programs?

Trade-off between flexibility for implementers vs. standardization for evaluation

Our Approach: Standardized Measurement of Outcomes, not Treatments

Common measurement framework across ALL programs:

1. **Digital skills:** Based on EU DigComp framework (DESI)
    - 12 core items (Likert 1-4), rescaled to 100
    - Measured at baseline and endline
2. **Soft skills:** Validated psychometric scales
    - 26 items covering openness, collaboration, leadership, empathy
    - Rescaled to 100
3. **Employment outcomes:** At 3 and 6 months post-training
    - Employment status (0/1)
    - Net salary (censored at 400-2500 euros)
    - Contract type, digital occupation

Constraint #3: Implementation Timing Mismatches

The problem:

- Beneficiaries start and finish training at *different times* (spread over 20 months)
- Panel surveyed at *regular intervals* (every 3 months)
- Simple matching on date creates small, unstable comparison groups

Example:

- Beneficiary finishes training January 2024
- Panel Wave 2: Dec 2023 - Feb 2024
- Which panel observations should we use as controls?

Challenge: Maximize control observations while maintaining temporal comparability

Compounding Timing Challenge: Timeline Expectations vs. Reality

What we expected (2022):

- Projects run for 15 months
- Measure outcomes at 3 and 6 months post-completion
- Complete evaluation by mid-2024

What actually happened:

- 18 of 23 projects received extensions (average: 4 months)
- Range: 0-5 months of extension
- Projects starting and ending at different times (spread over 20 months)
- Evaluation extended to September 2025

Our Approach: “Exploding” the Panel

Key insight: Panel members observed repeatedly $\rightarrow$ multiple potential controls

Method:

1. Each panelist measured 6 times (Oct 2023 - Sep 2025)
2. For each beneficiary finishing at time t:
    - Match to panelists in the *nearest wave*
    - Use their characteristics at time t (not entry)
3. “Explode” panel: each panelist creates multiple synthetic control observations

Example:

- Panelist observed in Wave 2, 3, 4
- Can serve as control for beneficiaries finishing in any of these periods
- Characteristics measured at the relevant wave (dynamic matching)

Result: 1,800 panelists $\rightarrow$ thousands of potential control observations

The Matching Trilemma

Three competing objectives:

1. **Common support**: Ensure treated and controls overlap on covariates
2. **Precise matching**: Match on many relevant characteristics
3. **Sample size**: Retain enough observations for power

Trade-off:

- More precise matching → better balance but smaller sample
- Coarser matching → larger sample but potential bias

Our Approach: Coarsened Exact Matching (CEM)

Main specification - Match on

- **Sampling cells:** Region (5 areas) × Gender × Age group (3) × Education (3) × Employment status
- **Timing:** Beneficiary matched with panel respondents over a period of similar duration
- **Job search motivation:** Active search (0/1) and Composite index of job-search intensity (willing to relocate, prefers employee and full-time)

Many robustness checks

- Finer geographic/demographic matching (region instead of area), family characteristics (children, welfare receipts), past training participation, nationality, etc.
- Choose main spec to ensure all 23 projects have sufficient common support

Identification: Difference-in-Differences

Estimating equation (First Difference):

$$\Delta Y_i = \alpha + \beta \cdot \text{Treated}_i + \epsilon_i$$

where $\Delta Y_i = Y_i^{\text{post}} - Y_i^{\text{pre}}$

Interpretation:

- Difference-in-Differences with two periods
- β: differential change between treated and matched controls
- Standard errors clustered at panelist level (repeated observations)

Concerns #1: Parallel trends

Key assumption: Parallel trends

$$E[\Delta Y_i(0)|D_i = 1, X_i] = E[\Delta Y_i(0)|D_i = 0, X_i]$$

When this may fail

- Time-varying selection on unobservables
- Differential exposure to other shocks
- Anticipation effects (e.g. lying to get into the panel)
- Ashenfelter's dip

Concerns #2: Selection on Observables vs. Unobservables

What we can address

- Selection on demographics (age, gender, region, education)
- Selection on employment status and job-search behavior
- Selection on stated interest in digital training
- Selection on baseline skills and motivation

What we cannot address (a lot!)

- Time-varying unobserved motivation
- Unobserved ability or talent
- Social networks and information channels

Cannot fully rule out bias from unobserved confounders: cost of not having ex-ante randomization

Summarizing: Ex-ante vs. Ex-post Evaluation Design

	What We Could Control	What We Could Not
Before programs start	• Outcome measures • Data collection timing • Baseline survey design • Evaluability requirements	• Treatment assignment • Program content • Participant selection • Implementation fidelity
During implementation	• External panel design • Follow-up surveys • Administrative data access	• Program extensions • Dropout decisions • Actual training delivered

Key Insight: Ex-post evaluation design allows some control over identification strategy, but within constraints set by program implementation

What Happened?

Implementation Overview

Final sample (as of September 30, 2025)

- **FUTURA (Women 18-50):** 11 projects, 1,722 target beneficiaries, 1540 completed
- **ONLIFE (NEETs 15-34):** 12 projects, 3,019 target beneficiaries, 2741 completed training

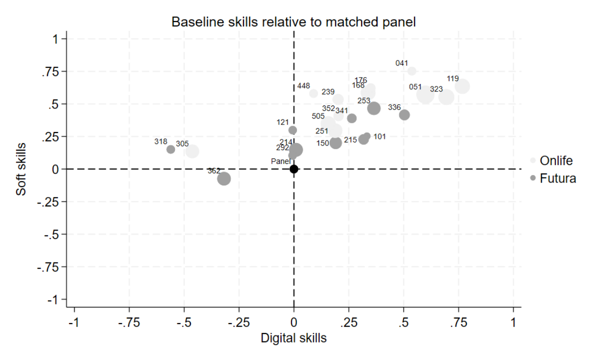
Program completion

- All 23 projects completed training activities by end of March 2025
- 6-month employment outcomes available as of September 2025
- Extensions: 18 of 23 projects (average 4 months)

Analysis uses projects with sufficient data quality and sample size

Selection Patterns

Who did programs select?



Note: differences are expressed in standard deviations with respect to matched panel controls for each project.

Key finding: Strong heterogeneity in baseline skills

- Some projects selected participants with *low* digital skills
- Others selected participants with *high* digital skills (above panel average)
- Reflects different program orientations:
  - Basic digital literacy vs. advanced technical training
  - Some projects: 0% with basic training; others: 60% basic

Dropout Heterogeneity

Dropout rates varied dramatically across projects:

	Range	Projects >25% dropout
FUTURA	12% - 39%	3 of 11
ONLIFE	7% - 55%	4 of 12

Why does dropout matter?

- Differential attrition can bias impact estimates
- Need to understand who drops out and why
- Affects interpretation of “treatment on the treated” vs. “intent to treat”

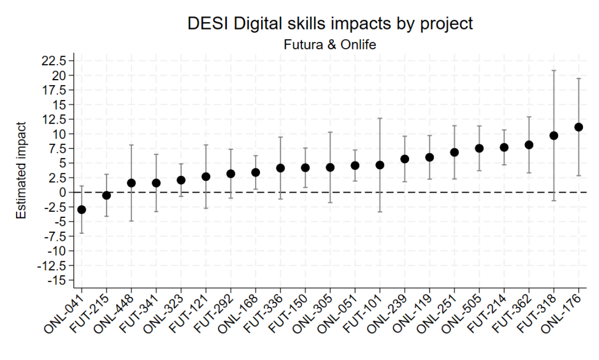
Survey response rates:

- Baseline: 84%-100% across projects
- Endline: 40%-100% (mean: 87%)
- 3-month follow up: 56%-98% (mean: 84%); 6-month: 56%-100% (mean: 79%)

Results: Aggregate Impact on DESI Digital Skills

	FUTURA (Women 18–50)		ONLIFE (NEETs 15–34)	
	Digital Skills (0–100)	Soft Skills (0–100)	Digital Skills (0–100)	Soft Skills (0–100)
	Digital	Soft	Digital	Soft
First Difference	4.24***	0.91	4.47***	1.73
	[0.98]	[0.74]	[0.96]	[1.48]
Mean change	9.72	2.12	7.47	2.40
Treated on support	1163 (88%)	1163 (88%)	1859 (77%)	1859 (77%)
Unique panel controls	876	876	1022	1022

The Real Story: Treatment Effect Heterogeneity



Key findings

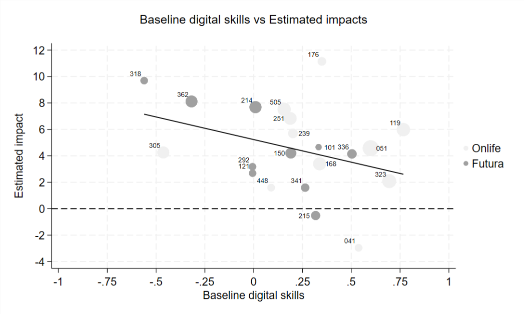
- Large variation across projects (0 to +9 points)

*Two projects excluded from the estimation because of too low response rates to the Endline survey (<50%)

What drives the heterogeneity across programs?

- **Strong negative correlation:** Baseline skills $\leftrightarrow$ Impact
- Projects selecting lower-skilled participants → Larger impacts
- Makes sense: More room to grow on basic DESI competencies
- Advanced technical training doesn't move DESI needle

Heterogeneity: Baseline Skills Drive Impacts



Note: differences in baseline skills are expressed in standard deviations with respect to matched panel controls.

Interpretation

- *Basic digital literacy* projects: large impacts (6–8 pts)
- *Advanced technical skills* projects: small/zero impacts on DESI

Important clarification

- This does **not** imply advanced training is ineffective
- DESI captures *basic* competencies only
- Advanced skills may affect employment outcomes not measured here

Lesson on External Validity: Program effectiveness varies widely—evidence matters for targeting and scaling.

Aggregate Employment Effects - 3 months

Table 1: Effects on Occupation and Wage after 3 Months

	Futura		Onlife	
	Occupation	Wage	Occupation	Wage
First Difference	0.06***	104.94***	0.07*	68.47
	[0.02]	[22.33]	[0.04]	[41.96]
Average change	0.11	172.81	0.29	288.46
Treated on support	1169 (91%)		1614 (70%)	
Unique panel controls	818		733	

Notes: Standard errors in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

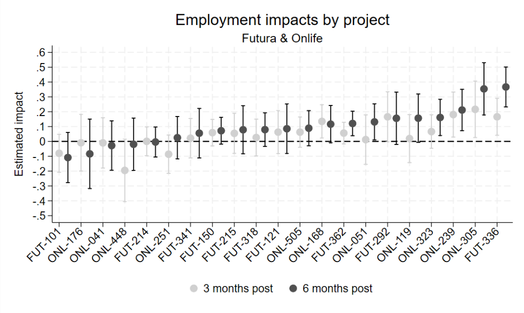
Aggregate Employment Effects - 6 months

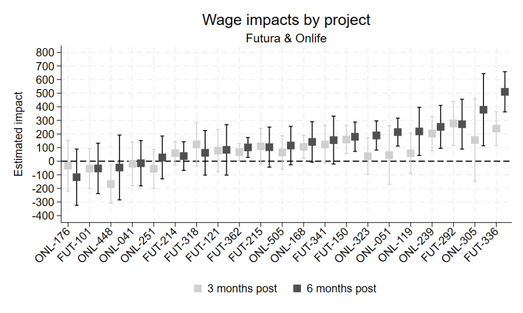
Table 2: Effects on Occupation and Wage after 6 Months

	Futura		Onlife	
	Occupation	Wage	Occupation	Wage
First Difference	0.10***	148.10***	0.13***	170.81***
	[0.02]	[27.08]	[0.04]	[38.67]
Average change	0.16	230.93	0.38	415.50
Treated on support	1121 (91%)		1662 (77%)	
Unique panel controls	778		649	

Notes: Standard errors in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The Real Story: Treatment Effect Heterogeneity





Robustness Checks: Core of the Analysis

Philosophy: Robustness is not an appendix—it's central to credibility

Five robustness checks on digital skills impacts:

1. **Add past training to match:** Controls for prior learning
2. **Use all 45 digital questions:** Not just 12 common items
3. **Drop multiple beneficiaries:** Individuals in >1 project
4. **Exclude foreign nationals:** Underrepresented in panel

Results:

- Most project-level effects robust across specifications
- Few projects lose significance with stricter matching
- Aggregate effects very stable

What Can We Conclude? Credible vs. Exploratory

Credible causal claims:

- ✓ Digital skills increased
- ✓ Employment increased
- ✓ Effects heterogeneous by baseline
- ✓ Effects robust to specification

Exploratory/uncertain:

- ? Long-run employment effects
- ? Which program features matter most
- ? Spillovers to families/networks
- ? Fiscal returns (MVPF)
- ? Optimal targeting rules

Evidence strength

- **Internal validity:** Reasonably strong (conditional on observables)
- **External validity:** Limited (specific programs, specific populations, specific context)
- **Mechanism:** Unclear (skills vs. credentials vs. networks vs. confidence)

Contextualizing Results in the Literature

How do our effects compare to gold-standard RCTs?

Program	Method	Earnings	Follow-up
Year Up	RCT	+25%	6-9 years
Per Scholas	RCT	+22%	6 years
Project QUEST	RCT	+15%	9 years
WorkAdvance	RCT	+10-15%	3 years
FRD - FUTURA	Quasi-exp	+31%	6 months
FRD - ONLIFE	Quasi-exp	+59%	6 months

Interpretation:

- Very large impacts
- Very low starting points (unemployed)
- Need longer follow-up to assess true returns (still in internships)
- Cost-effectiveness may be comparable

Lessons for Experimental Researchers

Lesson 1: Design for Evaluation Early (Even Without Full Control)

What we did right:

- Engaged with policymakers *before* program launch
- Pre-specified outcome measures and data collection
- Built external panel while programs were starting
- Standardized measurement across all programs
- Build in possibility of matching with Social Security data (ongoing analysis)

What we would do differently:

- Push harder for at least *some* randomization (pilot sites?)
- Pre-specify analysis plan more explicitly (though flexibility was needed)
- Collect richer baseline data on motivation and skills

Key Insight: early engagement lets you build in evaluation features even in constrained settings

Lesson 2: Heterogeneity Is Not a Bug, It's the Research Question

Traditional view:

- Estimate average treatment effect (ATE)
- Heterogeneity is nuisance, reduces power
- Subgroup analysis is exploratory

Modern view (and our experience):

- **Heterogeneity $\rightarrow$ Targeting:** Who benefits most?
- **Heterogeneity $\rightarrow$ Mechanism:** What features drive effects?
- **Heterogeneity $\rightarrow$ External validity:** How to generalize?

Frontier methods leveraging heterogeneity:

- **Optimal treatment assignment** (Kitagawa, Manski): Use heterogeneity to improve allocation
- **Experimental selection correction** (Athey, Chetty, Imbens 2024): Combine RCT + observational data

Lesson 3: Other Benefits

Unexpected benefits of the Project

- Building a motivated and engaged team of young researchers
- Being able to have data-driven strategic conversations with policy makers
- Building a public good

Why Transparency Matters

- Credibility to advice policy makers
- Review process

Next Frontier: MVPF and Welfare Analysis

Marginal Value of Public Funds (Hendren & Sprung-Keyser 2020):

$$\text{MVPF} = \frac{\text{Beneficiary willingness to pay}}{\text{Net government cost}}$$

What we need for MVPF calculation:

- ✓Program costs (€3,000 per beneficiary) — *We have this*
- ✓Impact on earnings — *We have this (short-term)*
- × Impact on taxes and transfers — *Need administrative data (INPS)*
- × Long-run persistence — *Need longer follow-up*

Next steps:

1. Link to tax/social security records (ongoing)
2. Compare MVPF to other education/training policies

Hendren framework enables welfare comparison across very different policies

Reflections

Implications for Researchers: Embedded Evaluation

Opportunity: Governments increasingly want rigorous evidence

What embedded evaluation offers:

- Access to large-scale policy experiments
- Real-world implementation (high external validity potential)
- Policy impact (research that matters)
- Novel research questions (mechanism design, optimal targeting)

A Possible Career Path

Embedded evaluation is increasingly common (J-PAL model, IPA, behavioral insights teams). This provides a viable career opportunity for research minded people

What it requires (skills beyond standard PhD)

- **Stakeholder management:** Working with non-researchers
- **Rapid turnaround:** Policy timelines $\neq$ academic timelines
- **Communication:** Translate technical findings for lay audience
- **Pragmatism:** Accept constraints, find creative solutions
- **Team building:** Train RAs, build institutional capacity

Implications for Policymakers: What Rigorous Evaluation Enables

What evaluation delivered for Fondo Repubblica Digitale:

1. **Evidence on what works:**
    - Identify effective programs for scale-up
    - Avoid scaling ineffective programs
    - Understand which populations benefit most
2. **Accountability:**
    - Demonstrate impact to funders and public
    - Justify continued investment
    - Build credibility for future initiatives
3. **Learning and adaptation:**
    - Understand implementation challenges
    - Refine program design based on evidence
    - Continuous improvement culture
4. **State capacity:**
    - Train cohort of researchers in evaluation methods
    - Build permanent evaluation capability: economies of scale

Open Research Questions (Dissertation Ideas!)

Emerging from this project:

1. **Optimal treatment assignment with heterogeneous effects**
   - Given our heterogeneity estimates, how should we allocate future programs?
2. **Combining experimental + observational data**
   - Some programs might do RCTs in scale-up phase
   - How to combine with our quasi-experimental estimates? (Athey, Chetty, Imbens 2024)
3. **Selection correction and external validity**
   - How to correct for selection when scaling to new contexts?
4. **Long-run effects and lifecycle impacts**
   - Persistence of skill gains; Career trajectories, family spillovers
5. **Mechanisms**
   - Why are some programs effective and others not?

Conclusion: The Future of Applied Micro in Policy Settings

1. **Rigorous evaluation is possible even without ideal experimental design**
    - Creative methods: external panels, interest-based screening, exploding observations
2. **Real-world constraints drive methodological innovation**
    - Each constraint → an opportunity to develop new approaches
    - Quasi-experimental methods are evolving rapidly
3. **Heterogeneity is the frontier**
    - Treatment effect heterogeneity → targeting, mechanism, generalization
    - Machine learning + causal inference = powerful combination
4. **Embedded evaluation offers unique research opportunities**
    - Impact at scale
    - Novel questions at research-policy interface
    - But requires different skills than traditional academic work

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Questions & Discussion

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Interested in collaboration?

The Fondo Repubblica Digitale evaluation is ongoing.
Opportunities for PhD students and postdocs based in Italy.

Backup: Balance Checks

Table 3: Covariate Balance after matching

	Futura			Onlife		
	Endline	3 months	6 months	Endline	3 months	6 months
Duration btw baseline and endline/follow-up	-2.57	9.00*	22.45***	-8.82***	4.78	6.29
Age	0.06	0.46	0.39	-0.31	-0.04	-0.20
Max education level	0.01	0.06	0.07	0.09	0.06	0.03
Actively searching for job (0/1)	-0.00	-0.00	-0.00	0.00	0.00	0.00
Training in last 3 months	-0.01	-0.07***	-0.07***	-0.01	-0.03	-0.03
Not working for family reasons	0.00	-0.03	-0.03	0.00	-0.01	-0.00
Children (0/1)	-0.01	0.00	0.01	-0.04**	-0.03*	-0.04*
Foreign (0/1)	0.05***	0.05***	0.04***	0.06***	0.06***	0.07***
Household support measure	0.06***	0.05***	0.05**	-0.05	-0.04	-0.00
Baseline wage	-142.88***	-128.42***	-122.29***	-13.39**	-33.26**	-30.33**
Baseline digital skills (0–100)	1.30	-4.67***	-4.40***	6.12***	-1.41	-0.80
Baseline soft skills (0–100)	2.37***	1.82***	1.52**	5.68***	6.12***	4.85**

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Variables included in the matching in bold.

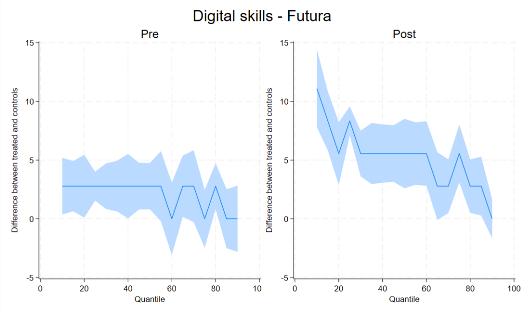
Backup: Common Support % by Project

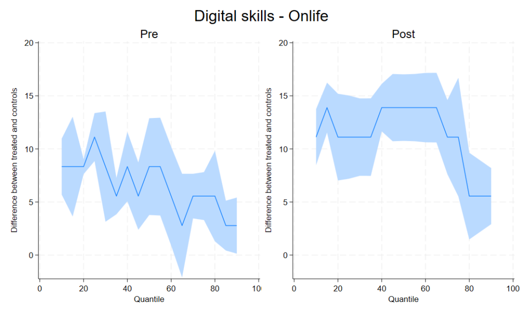
Futura			
	End	3m	6m
Aggregate	88	91	91
2022-FUT-00101	88	81	87
2022-FUT-00121	86	97	97
2022-FUT-00150	87	92	92
2022-FUT-00214	89	89	90
2022-FUT-00215	89	95	98
2022-FUT-00253	90	89	90
2022-FUT-00292	89	95	95
2022-FUT-00318	78	72	71
2022-FUT-00336	87	87	85
2022-FUT-00341	85	94	93
2022-FUT-00362	91	96	95

Onlife			
	End	3m	6m
Aggregate	77	70	77
2022-ONL-00041	83	80	90
2022-ONL-00051	79	65	73
2022-ONL-00119	69	48	68
2022-ONL-00168	91	83	90
2022-ONL-00176	84	75	77
2022-ONL-00239	83	79	86
2022-ONL-00251	71	68	65
2022-ONL-00305	49	68	79
2022-ONL-00323	82	70	76
2022-ONL-00352	74	87	92
2022-ONL-00448	72	80	87
2022-ONL-00505	78	78	75

Note: the table presents the percentages of common support for each project in the different two periods comparison (Baseline- Endline, Baseline - 3 months, Baseline - 6 Months).

Backup: Quantile Regression Results





Note: the table presents quantile regression on digital skills for treated beneficiaries versus matched panel controls both before and after the training.

Project Characteristics and Performance

Project ID	Target	Area	Grant (€)	Duration	Extension	Trained	Dropout (%)	% Target	Base (%)	End (%)	3m. (%)	6 m. (%)
FUT-00101	150	Piemonte	471650	15	3	50	21	33	100	84	86	78
FUT-00121	100	Toscana	343140	12	5	75	21	75	100	92	88	85
FUT-00150	150	National	384694	15	5	157	39	105	96	91	88	88
FUT-00214	250	National	713432	15	3	251	18	100	99	89	75	69
FUT-00215	120	Sicilia	252214	15	5	117	26	98	99	91	85	71
FUT-00253	214	National	635762	15	5	196	28	92	100	68	76	78
FUT-00292	143	Marche	363836	15	5	89	18	62	87	88	69	69
FUT-00318	120	Emilia-Romagna	300775	15	4	98	22	82	84	51	67	63
FUT-00336	150	National	516138	15	0	147	12	98	100	100	97	92
FUT-00341	100	Calabria, Sicilia	250000	15	0	105	17	105	100	99	93	87
FUT-00362	225	Calabria, Campania, Puglia	587132	15	5	255	12	113	100	96	95	93
ONL-00041	100	Campania	278982	15	3	94	7	94	100	100	89	83
ONL-00051	500	National	963185	12	0	492	12	98	100	92	85	79
ONL-00119	333	National	980093	15	0	328	10	98	100	99	91	89
ONL-00168	300	Puglia	816481	15	0	223	38	74	100	100	96	85
ONL-00176	125	Sicilia	351635	15	1	93	32	74	100	99	90	85
ONL-00239	150	Puglia, Campania	374125	15	5	151	18	101	99	87	79	75
ONL-00251	240	Centre-North regions	550840	15	4	240	29	100	99	80	78	70
ONL-00305	360	Toscana, Marche, Lazio	936055	15	5	281	15	78	89	60	56	56
ONL-00323	330	National	977711	15	1	334	17	101	100	100	93	88
ONL-00352	140	Campania	390680	15	5	131	15	94	96	40	66	56
ONL-00448	107	Campania	296300	15	5	47	55	44	100	100	98	100
ONL-00505	334	National	916722	15	5	327	10	98	100	97	92	87

Project Content

Project ID	Basic (%)	Advanced (%)	Courses Offered
FUT-00101	19	81	UX Design; Web Design; Cloud & Data Protection; Software Dev
FUT-00121	0	100	Social Media; AR; E-business; 3D Printing
FUT-00150	38	62	IT Base; Adv. Excel; Low/No Code; PHP/JS; Web Dev; HTML/CSS; React; SMM; Wordpress, Canva
FUT-00214	16	84	Basic/Advanced; SMM; Multimedia; Web/Graphic Design; Front/Back-end
FUT-00215	0	100	Digital Marketing; E-Commerce; Digital Fundraising
FUT-00253			Modular training
FUT-00292	37	63	Computer Literacy; SMM; Design Thinking; PM; UX; Web/Mobile; Emerging Tech
FUT-00318	43	57	Digital Literacy; Reskilling; Upskilling
FUT-00336	17	83	Java Dev; Data Engineer
FUT-00341	9	91	Data Science Camp; Bootcamp; Data Weekenders
FUT-00362	46	54	IT Basics; Digital Graphics; E-commerce; Full-stack
ONL-00041	0	100	VR Development
ONL-00051	69	31	Level I; Full Stack; Java
ONL-00119	17	83	Jr Java; Cybersec; Data Eng; Salesforce
ONL-00168	79	21	Digital Marketing; Cybersecurity; UX
ONL-00176	33	67	Front-end; Back-end
ONL-00239	35	65	Coding; Cybersec; Digital Marketing; Web Data Sci; SMM
ONL-00251	12	88	Base+Adv; Cybersec Arch.; Data Analyst; Web Design
ONL-00305	46	54	Modular training
ONL-00323	0	100	Front-end; Full Stack; Data Analyst; Cybersec; IT Spec.
ONL-00352	48	52	Modular training
ONL-00448	12	88	Digital Skills; Web & Social Marketing; Cybersec; Java
ONL-00505	21	79	Digital Marketing; Web & E-com.; CAD Fashion; Web/App Dev; IT Sec.