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Are Temporary Jobs Stepping Stones or Dead Ends? A Meta-Analytical Review of the Literature

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ABSTRACT

Are Temporary Jobs Stepping Stones or Dead Ends? A Meta-Analytical Review of the Literature*

We present a meta-analysis on the debate about the "stepping stone vs. dead end" hypothesis related to the causal effect of temporary jobs on future labour market performances. We select academic papers published on international peer-reviewed journals from 1990 until 2021. Among 78 observations from 64 articles, 32% support the hypothesis according to which temporary contracts are a port of entry into stable employment positions, 23% report ambiguous or mixed findings, and the remaining 45% provide evidence in favour of the dead end hypothesis. The results from meta-regressions suggest that the stepping stone effect is more likely to emerge when self-selectivity issues are dealt with, especially when using the timing-of-events approach. The studies focusing on temporary work agency jobs and casual/seasonal jobs detect more easily results in favour of the dead end hypothesis. Finally, in more recent years and when the unemployment rate is larger, the dead end hypothesis is more likely to prevail.

JEL Classification: J08, J41, J42, J81

Keywords: meta-analysis, labour market, temporary jobs, stepping stones, dead ends

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1 Introduction

In the last three decades the labour market institutions of many OECD countries have changed substantially, with the rise of new forms of contracts, especially temporary forms of employment. In most cases, these reforms have left unchanged the employment protection of the standard open-ended contracts (OECD, 2004). Many studies have investigated the implications of these institutional changes both from the macroeconomic viewpoint and in terms of individuals' labour market prospects.

The macroeconomic literature has focused on the relationship between the Employment Protection Legislation (EPL) index, calculated by the OECD and reflecting the degree of labour market flexibility, and aggregate employment and unemployment. Howell et al. (2007) report that most of the studies find no statistically significant relationship between these variables. The reviews in Boeri and Van Ours (2013) and Skedinger (2010) provide inconclusive results. Brancaccio et al. (2020) is the first meta-analysis about these relationships, using 53 papers published between 1990 and 2019: 28% of the papers find that the labour market deregulation increases employment and reduce unemployment; the remaining articles report either ambiguous results (21%) or a negative impact on labour market outcomes (51%).

From the point of view of employment prospects at individual level, many authors have provided evidence on the so-called "stepping stone vs. dead end" debate, by estimating at individual level the effect of experiencing temporary or atypical jobs on the subsequent career in terms of employment satisfaction, job stability, and earnings. Although many studies have been published on this issue, to the best of our knowledge there is no analytical economic survey on the "stepping stone vs. dead end" debate. Only de Graaf-Zijl (2005) offered an overview on the literature dealing with the economic and social consequences of precarious job positions. However, the mid of the 2000s was still the onset of the literature on the stepping stone effect and many articles and new findings have been published since the review in de Graaf-Zijl (2005). The main contribution of our paper is to fill this gap and provide a comprehensive review of the results obtained so far on the impact of temporary jobs on future labour market performances at individual level.

The empirical literature does not provide clear-cut findings on the debate around temporary employment and its consequences on individuals' subsequent labour market outcomes. Some studies support the stepping stone hypothesis (e.g. Addison and Surfield,

2009; Buddelmeyer and Wooden, 2011; de Graaf-Zijl et al., 2011; Ichino et al., 2008; Picchio, 2008; Cockx and Picchio, 2012). Others find that temporary jobs are a trap, rather than a bridge to open-ended contracts (e.g. Alba-Ramirez, 1998; Amuedo-Dorantes, 2000; Böheim and Weber, 2011; García-Pérez et al., 2019). Moreover, there are also ambiguous or controversial pieces of evidence, either because temporary positions are found to have an insignificant effect (e.g. Barbieri and Cutuli, 2016; Esteban-Pretel et al., 2011; Freier and Steiner, 2008), or because temporary jobs are found to be stepping stones towards stable positions but generating lower wages in the future (Booth et al., 2002; Boockmann and Hagen, 2008; Addison et al., 2015). Finally, there are papers reporting evidence that could be in favour of the stepping stone hypothesis, but only if the worker does not experience repeated flexible contracts or job interruptions (Gagliarducci, 2005; Sanz, 2011). In light of these conflicting and different findings, a second contribution of our paper is to quantitatively combine evidence from different studies on a similar theme by way of meta-analysis techniques, so as to provide a systematic review and understanding of heterogeneous results about the impact of temporary jobs on subsequent labour market performances. Our analysis is based on a meta-analysis using 64 articles published in international peer-reviewed journals. Our meta-analysis follows an approach similar to the one in Kluve (2010).

The remainder of the article is organized as follow: Section 2 provides a theoretical background about the "stepping stone vs. dead end" debate. Section 3 describes the selection criteria to generate the sample used in the meta-analysis. Section 4 reports descriptive statistics on the sample and how different characteristics of the studies correlate to the research outcomes. Section 5 shows and comments on the results from meta-regressions. Section 6 concludes.

2 Theoretical background

The stepping stone vs. dead end debate emerges from ambiguous predictions provided by the economic theory. On the one hand, according to the stepping stone hypothesis, temporary jobs may be of help in obtaining more stable and better positions, especially for those belonging to disadvantaged groups, who otherwise would have been excluded from the labour market by too strict regulations. The main channels are the accumulation of work experience and human capital, the access to social networks, the signaling of high motivation (Loh, 1994; Wang and Weiss, 1998), and the accumulation of financial assets

to sponsor a longer and better job search in the eventual subsequent unemployment spell (Browning et al., 2007).

On the other hand, employers might use temporary contracts as a mere flexibility buffer. If so, temporary workers could have low chances of getting the conversion to a stable position, have a discontinued career made up of a repetition of short-term and low paid jobs. Furthermore, since temporary workers are more likely to leave the firm sooner and employers' incentives to invest in training are negatively related to the probability of job mismatch (Acemoglu and Pischke, 1998), they will be less likely to receive firm-sponsored training and will have a smaller incentive to investing in their own human capital (Arulampalam and Booth, 1998; Albert et al., 2005; Fouarge et al., 2012). As a matter of fact, Blanchard and Landier (2002) concluded that labour market reforms in France have substantially increased turnover in fixed-term jobs, without a reduction in unemployment duration or a positive impact on welfare of young workers.

To complicate theoretical predictions, it should be considered that other factors could play a relevant role by interacting with temporary employment, among which labour market institutions. As pointed out by Casquel and Cunyat (2008), firms decide to keep a worker in a permanent job only if the surplus generated under a permanent contract is greater than the firing costs. But, what determines whether the stepping stone effect dominates or not is the value of productivity exceeding a threshold productivity value, which depends also on the institutional labour market regulation. In particular, an increase in the unemployment benefits, in the firing costs, or in the set-up costs determines an higher threshold value. Moreover, when the firing costs of permanent workers are large, temporary jobs could be more intensively used as a screening device, rather than a buffer to face the business cycle, because firms give a larger importance to the assessment of the quality of workers before signing an open-ended contract. At the same time, large firing costs for permanent workers could exacerbate the use of temporary position as a flexibility buffer (Cockx and Picchio, 2012; Tejada, 2017) to face product demand volatility, giving rise to a duality in the labour market, with the secondary market based on short-term relationships and populated by the most disadvantaged groups, like the youth or women.

Finally, the effect of temporary jobs can be different because of the interactions with other contract types, for example with those involving firm-provided training, such as the apprenticeship. When these types of contracts are available and favoured by the regulations (e.g. reduction in employers' labour costs), firms could prefer to use them to induce self-selection of more able workers and facilitates worker screening (Autor, 2001; Picchio

and Staffolani, 2019), instead of using fixed-term contracts, hence becoming more likely to be relegated to the function of a buffer to face cyclical downturns.

3 Meta-analytic sample selection criteria

The empirical literature does not show clear-cut results on the stepping stone hypothesis for temporary jobs. Several reasons could explain the different findings, such as different samples, identification strategies, and methodological tools. Moreover, as explained in Section 2, an important role is theoretically played by different labour market institutions across countries. Hence, a simple comparison of the different studies and results could be misleading (Stanley et al., 2013). A meta-analysis can avoid such problems.

Our research selection process started by searching articles in Google Scholar, Ideas, Scopus, and Web of Science databases in the first week of March 2021. We used a combination of the following keywords: “temporary job”, “stepping stone” and “dead end”, joined by “and". We then checked by adding new keywords, such as “temporary employment” and “atypical work”. Moreover, we applied filters in order to consider only articles published from 1990 in peer-reviewed journals dealing with labour, economics, political sciences and sociology and with the SCImago Journal Rank (SJR) indicator.[1] As further exclusion criteria, we removed all theoretical works, all macroeconomic analyses, all studies focused on EPL and empirical analyses that draw their conclusions on the effects of temporary jobs by considering labour market reforms and EPL dynamics. These studies are different from our research purpose: we only focus on microeconomic analyses which study whether the choice of accepting a temporary contract is a port of entry into stable employment positions or a dead end. We include studies independently on the methodological approach to identify the causal effect: one of the objectives of our meta-analysis is to investigate if the identification/estimation strategy is a key factor in explaining different findings.

Our search returned a final sample of 64 papers, which are listed in Table 1, where we provide study-related characteristics and a brief description of the research design of each study. However, it is possible that a given article evaluates more than a type of temporary contract, i.e. it yields two or more data points. In this way, our sample reaches 78 observations, belonging to the 64 studies analysed. As regard the classification

[1] See www.scimagojr.com/SCImagoJournalRank.pdf for details on the calculation of the SJR.

procedure of the research outcomes, we assign to each study, on the basis of the results that are statistically significant and authors' interpretation of their findings, one of the following three outcomes: i) works with empirical evidence in favour of the stepping stone hypothesis; ii) articles supporting the dead end hypothesis; iii) papers providing mixed, controversial, or no significant effects.[2]

The distribution of this outcome variable taking three discrete values is as follows: 25 (32%) results support the stepping stone hypothesis, 35 (45%) observations support the dead end hypothesis, suggesting the entrapment or scarring effect of temporary jobs, and 18 (23%) findings provide mixed or controversial evidence. This descriptive picture would not change if we counted studies not published in refereed journals, articles published in journals without the SJR indicator, working papers, and book chapters: 6 studies consider temporary jobs as a port of entry into permanent employment (e.g. Andersson et al., 2009; Böheim and Cardoso, 2009), 7 papers support the entrapment effect into unemployment or recursive temporary jobs (e.g. Heinrich et al., 2009; Autor and Houseman, 2006; Dekker, 2008), and 3 papers report mixed results (Verhofstadt and Goebel, 2008; Kvasnicka, 2009; Hopp et al., 2016). If we added these 16 findings to the previous list of 78 observations, we would have 33% of the manuscripts supporting the stepping stone hypothesis, 45% in favour of the dead end, and 22% with mixed or controversial findings.

[2]By "controversial" we mean studies: a) supporting neither the stepping stone nor the dead end hypothesis; b) highlighting scars in terms of lower subsequent wages; c) supporting the stepping stone hypothesis, but only if there are no repeated temporary contracts or interruptions.

Table 1: Articles included in the meta-analysis ($N = 64$)

Authors	Journal	Google Scholar citations on 08/03/21	Country	Data (Time Span)	Sample size	Method	Hypothesis supported
Addison and Surfield (2009)	Applied Economics	42	USA	CPS/CAEAS (1997-2005)	Medium	CF	Stepping stone
Addison et al. (2015)	Manchester School	11	USA	NLSY (1992-1998)	Large	PSM	Stepping stone
Ahn (2016)	International Labour Review	1	South Korea	KLIPS (1998-2012)	Medium	DREP	Dead end
Alba-Ramirez (1998)	Journal of Labor Research	165	Spain	SAP (1987-1996)	Large	CF	Dead end
Amuedo-Dorantes (2000)	ILR Review	333	Spain	LFS (1995-1996)	Large	Multin. logit, DM	Dead end
Amuedo-Dorantes et al. (2008)	Journal of Labor Research	81	Spain	Admin. Data (1998-2004)	Large	PSM	Dead end
Auray and Lepage-Saucier (2021)	Labour Economics	0	France	FH-DADS (1996-2004)	Large	ToE	Stepping stone
Autor and Houseman (2010)	American Economic Journal	311	USA	Work First Data (1999-2003)	Large	OLS, IV	Dead end
Babos (2014)	Post-Communist Economies	14	8 East-EU countries	EU-SILC (2005-2010)	Medium	CF	Dead end
Baranowska-Rataj et al. (2011)	Work, Employment and Society	74	Poland	PSLS (1998-2005)	Large	DM	Mixed
Barbieri and Sestito (2008)	Labour	87	Italy	LFS (1993-2003)	Large	PSM	Stepping stone
Barbieri and Cutuli (2016)	European Sociological Review	112	13 EU countries	EU-LFS (1992-2008)	Medium	FE	Mixed
Barbieri and Cutuli (2018)	De Economist	7	Italy	SHIW (2004-2014)	Medium	DREP	Dead end
Barbieri et al. (2019)	Socio-Economic Review	22	Italy	MSIH (1980-2009)	Large	DREP	Dead end
Berglund et al. (2017)	Nordic Journal of Working Life Studies	29	Sweden	LFS (1992-2010)	Large	Multin. Logit	Mixed*
Berson (2018)	De Economist	6	France	LFS, PIAAC, WDN (2003-2016)	Large	Multin. Probit, Heckprobit	Dead end
Berton et al. (2011)	International Journal of Manpower	141	Italy	WHIP (1998-2004)	Medium	FE	Stepping stone
Böheim and Weber (2011)	German Economic Review	32	Austria	ASSD (1993-2001)	Large	PSM	Dead end
Boockmann and Hagen (2008)	Labour Economics	122	Germany	GSOEP (1985-2002)	Medium	PSM	Stepping stone
Booth et al. (2002)	Economic Journal	1,561	UK	BHPS (1991-1997)	Medium	DM	Stepping stone (FTCs), Dead end (Casual jobs)
Bosco and Valeriani (2018)	Italian Economic Journal	1	Italy	Admin. Data (2008-2015)	Large	PSM	Dead end
Bosio (2011)	Politica Economica	6	Italy	CLAP (1992-2002)	Large	DM	Mixed
Bruno et al. (2013)	Rivista Internazionale di Scienze Sociali	29	Italy	IT-SILC (2004-2007)	Medium	DM	Dead end
Buddelmeyer and Wooden (2011)	Industrial Relations	51	Australia	HILDA (2001-2005)	Large	DREP	Stepping stone
Chalmers and Kalb (2001)	Australian Economic Review	48	Australia	SEUP (1994-1997)	Medium	ToE	Mixed
Cockx and Picchio (2012)	Oxford Bulletin of Econ. and Stat.	123	Belgium	CBSS (1998-2001)	Large	ToE	Stepping stone
D'Addio and Rosholm (2005)	Labour Economics	160	EU members	ECHP (1994-1999)	Large	Multin. logit	Dead end
de Graaf-Zijl et al. (2011)	Journal of Population Economics	349	Netherlands	OSA-LSP (1988-2000)	Small	ToE	Stepping stone
De Lange et al. (2014)	Economic and Industrial Democracy	31	Netherlands	OSA-LSP (1986-2008)	Small	CF	Mixed
Esteban-Pretel et al. (2011)	Labour Economics	59	Japan	ES Survey (2002)	Medium	Other method	Mixed
Freier and Steiner (2008)	Journal of Labour Market Research	42	Germany	EP-FEA (2001-2002)	Large	PSM	Mixed
Fremigacci and Terracol (2013)	Applied Economics	35	France	Admin. Data (2001-2004)	Large	ToE	Stepping stone
Gagliarducci (2005)	Labour Economics	258	Italy	ILFI (1997)	Medium	ToE	Mixed
García-Pérez and Muñoz-Bullón (2011)	British Journal of Ind. Relations	108	Spain	SSS Records (1996-2003)	Large	DM	Dead end
García-Pérez et al. (2019)	Economic Journal	51	Spain	MCVL (2006-2012)	Large	RDD	Dead end
Gash (2008)	European Sociological Review	232	4 EU countries	ECHP (1995-2001)	Medium	DM	Mixed
Giesecke and Groß (2003)	European Sociological Review	262	Germany	GSOEP (1984-1999)	Large	RE	Dead end
Givord and Wilner (2015)	Journal of Applied Econometrics	44	France	LFS (2002-2010)	Large	FE	Stepping stone (FTCs), Dead end (THA)
Hartman et al. (2010)	Empirical Economics	23	Sweden	Admin. Data (2002-2003)	Small	PSM	Mixed
Högberg et al. (2019)	Journal of Sociology	4	18 EU countries	EU-SILC (2004-2013)	Large	Other method	Stepping stone
Hveem (2013)	IZA Journal of Migration	28	Sweden	LISA (1997-2008)	Large	DiD	Dead end
Hyatt and Spletzer (2017)	Labour Economics	37	USA	LEHD, CPS (1996-2012)	Large	Stock-flows analysis	Stepping stone

(continued on next page)

Table 1: *Continued from previous page*

Authors	Journal	Google Scholar citations on 08/03/21	Country	Data (Time Span)	Sample size	Method	Hypothesis supported
Ichino et al. (2008)	Journal of Applied Econometrics	690	Italy	Manpower (2001)	Medium	PSM	Stepping stone
Jahn and Rosholm (2013)	Economics Letters	42	Denmark	Admin. Data (1997-2006)	Large	ToE	Stepping stone
Jahn and Rosholm (2014)	European Economic Review	48	Denmark	Admin. Data (1997-2006)	Large	ToE	Mixed
Kiersztyn (2020)	Acta Sociologica	1	Poland	POLPAN (2008-2013)	Small	CF	Dead end
Lane et al. (2003)	Journal of Policy Analysis and Manag.	94	USA	SIPP (1990-1993)	Large	PSM	Stepping stone
McGinnity et al. (2005)	European Sociological Review	174	Germany	GLHS (80s-90s)	Medium	Multin. logit	Mixed
McVicar et al. (2019)	European Sociological Review	14	Australia	HILDA (2001-2014)	Medium	Sequence analysis	Mixed
Mooi-Reci and Dekker (2015)	British Journal of Ind. Relations	22	Netherlands	OSA-LSP (1980-2000)	Medium	DREP	Dead end
Muehlberger and Pasqua (2009)	Review of Social Economy	38	Italy	ILFS (2004)	Large	Multin. logit	Dead end
Nunziata and Staffolani (2007)	Scottish Journal of Pol. Economy	107	15 EU countries	LFS (1983-1998)	Medium	FE, DiD	Stepping stone (FTCs), Dead end (THA)
Okudaira et al. (2013)	Journal of the Japan. and Int. Ec.	12	Japan	TWS (2008-2010)	Small	PSM	Dead end
Passaretta and Wolbers (2019)	Economic and Industrial Democracy	20	17 EU countries	LFS (1995-2009)	Large	Multin. logit	Dead end
Pavlopoulos (2013)	Review of Social Economy	18	UK, Germany	BHPS, GSOEP (1984-2008)	Medium	CF	Mixed
Picchio (2008)	Labour	107	Italy	SHIW (2000-2004)	Medium	DREP	Stepping stone
Sanz (2011)	Manchester School	19	Spain	LWLS (2000-2007)	Large	ToE	Mixed
Scherer (2004)	Work, Employment and Society	437	Germany, Italy, UK	GSOEP, BHPS, ILFI (1983-1998)	Medium	DM, RE	Mixed (FTCs), Dead end (Other jobs)
Steijn et al. (2006)	Work, Employment and Society	99	Netherlands	FSDP (2000)	Medium	Logit	Stepping stone
Svalund and Berglund (2018)	European Journal of Ind. Relations	22	Norway, Sweden	LFS (2000-2008)	Large	Logit	Dead end
Van den Berg et al. (2002)	Journal of Population Economics	68	Netherlands	Basic Doctor Data (1984-1990)	Medium	ToE	Stepping stone
Van Ours (2004)	Journal of Comparative Economics	257	Slovakia	Admin. Data (1993-1998)	Large	ToE	Mixed
Watson (2013)	Journal of Industrial Relations	65	Australia	HILDA (2001-2009)	Large	Multin. logit	Dead end
Yu (2012)	Social Forces	53	Japan	SSSMS (2005)	Medium	DM	Dead end

Notes: CF = Control Function; PSM = Propensity Score Matching; DREP = Dynamic Random Effects Probit; ToE = Timing of Events; OLS = Ordinary Least Squares; IV = Instrumental Variables; DM = Duration Model; FE = Fixed Effects; Multin. logit= Multinomial logit; RDD = Regression Discontinuity Design; DiD = Difference-in-Differences; RE= Random Effects; QR = Quantile Regression. Sample size: small means $N < 1,000$; medium means $1,000 \leq N < 10,000$; large means $N \geq 10,000$.

* Berglund et al. (2017): Substitute and probationary contracts = Stepping stones; Temporary jobs = Mixed; Seasonal, on-call, project and summer-works = Dead ends.

4 Univariate analysis

4.1 Study outcome by journal and publication features

We provide in this section some basic descriptive statistics of the articles in our sample by research findings. We focus first on characteristics like the journal subject area, the year of publication, the number of citations on average per year, and the journal SJR indicator at the time of publication.

Table 2 reports the average number of citations per year according to Google scholar (retrieved on 08/03/2021) and the SJR indicator at the time of publication by research outcome.[3] Although the dead end hypothesis is supported by the largest number of empirical analyses, the average number of yearly citations received by articles in favour of the stepping stone hypothesis (12.2) is 26% larger than the number of citations obtained by articles finding that temporary jobs are dead ends (9.7). The number of citations of articles reporting mixed or controversial results is even lower (8.85). These important differences in citations do not seem to justify the systematic differences in the scientific influence and prestige of the journals of publication, which could be an approximative measure of the scientific reliability of study results. The average value of the SJR indicator is indeed very similar across the three groups of research outcome, with the only difference that the SJR indicator of articles finding that temporary jobs are dead ends displays a larger standard deviation. It is noteworthy that articles providing mixed or controversial results are not underrepresented in journals of high prestige and scientific influence compared to those with more clear-cut findings. This might suggest that in this topic the publication bias (Sterling, 1959)[4] is not an issue.

Figure 1 reports the research outcomes by 4 journal subject areas according to the Scimago classification: i) Economics, Econometrics, and Finance; ii) Social sciences; iii) Business, Management, and Accounting; iv) a residual category containing journals belonging to multiple subject areas.[5] Figure 1 shows that almost all the observations

[3] At the time of publication, some journals did not have the SJR index yet, either because they were published in too recent years or because the journal was not indexed yet in Scimago. In these cases, we assign to the journal the first available SJR index. More in detail, we assigned to Alba-Ramirez (1998) the 1999 SJR index, to Auray and Lepage-Saucier (2021) the 2019 SJR index, to Berglund et al. (2017) the 2018 SJR index, to Freier and Steiner (2008) the 2010 SJR index, and to Kiersztyn (2020) the 2019 SJR index.

[4] For more recent works on publication bias, see Stanley (2005) and articles cited there in.

[5] Although the journal Social Forces is classified in Scimago in two subject areas, i.e. Social sciences and Art, and since Art is quite distant from the socio-economic issue analysed in this meta-analysis, we

Table 2: Descriptive statistics on article citations and SJR

	Stepping Stone	Dead End	Mixed/Controversial
a) Number of citations per year on 08/03/2021 (Google scholar)			
Mean	12.216	9.727	8.854
Standard deviation	18.421	14.445	7.531
Minimum	0.000	0.200	0.600
Maximum	82.158	82.158	25.706
b) SJR at the time of publication[(a)]			
Mean	0.939	1.055	1.056
Standard deviation	0.669	1.188	0.801
Minimum	0.172	0.111	0.111
Maximum	2.331	5.453	2.664
Observations	25	35	18

Source: Data retrieved from Google Scholar and Scimago Institutions Rankings on 08/03/2021.
[(a)] At the time of publication, some journals did not have the SJR index yet, either because they were published in too recent years or because the journal was not indexed yet in Scimago. Footnote 3 explains how we deal with these cases of missing information.

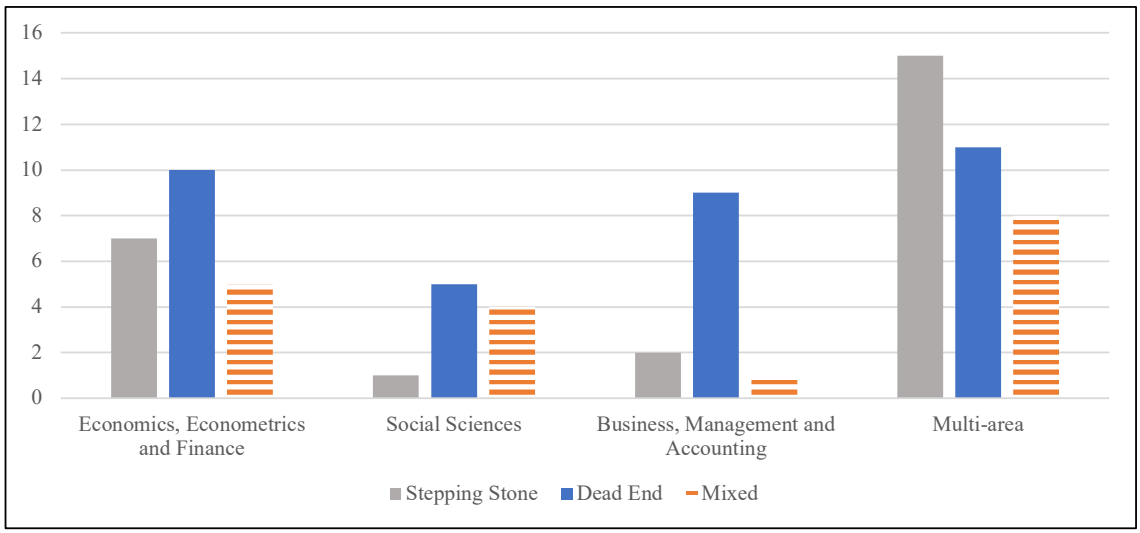
with findings supporting the stepping stone hypothesis come from journals in Economics, Econometrics, and Finance and from journals in multiple subject areas. At a first descriptive level, we can speculate that the journal subject area could be a relevant determinant in explaining different research outcomes.

To understand the relation between year of publication, which is a kind of (rough) approximation of the time period analysed in each article, and paper findings, we report in Table 3 the distribution of the absolute frequencies by decades and research outcomes. We also plot in Figure 2 the cumulative absolute frequency over publication years by research outcomes. Table 3 shows that the observations supporting the stepping stone hypothesis are equally divided between the last two decades. Instead, in the last decade, the number of articles providing evidence for the entrapment effect or mixed/controversial results more than doubles. Figure 2 visually clarifies that while the stepping stone and the mixed curves tend to become flatter in the last decade, the profiles of the cumulative distribution for the dead end hypothesis becomes steeper, especially in the last 4 years. Although the numerous labour market reforms reducing the EPL of permanent jobs after the onset of the Great Recession,[6] which decreased therefore the relative benefit of using

included Social Forces into the subject area Social sciences, as if it were classified in only one subject area in Scimago.

[6] Adascalitei and Morano (2016) count at least 642 changes in 110 developed and developing countries between 2008 and 2014. The majority of these interventions were implemented in the area of regular contracts and in the regulation of collective bargaining towards a reduction of the existing level of regulation.

Figure 1: Research outcomes by journal Scimago subject area



Notes: "Multi-area" comprises journals included in more than one Scimago subject area.

temporary jobs as a buffer, the evidence for the dead end hypothesis has become prevalent at the time of writing. It might be that more unstable and stagnant economic conditions, as those that followed the Great Recession, largely favoured the use of temporary jobs as a buffer, more than compensating the opposite effect induced by the concomitant reduction in the EPL for permanent workers.

Table 3: Summary statistics on research outcomes over time

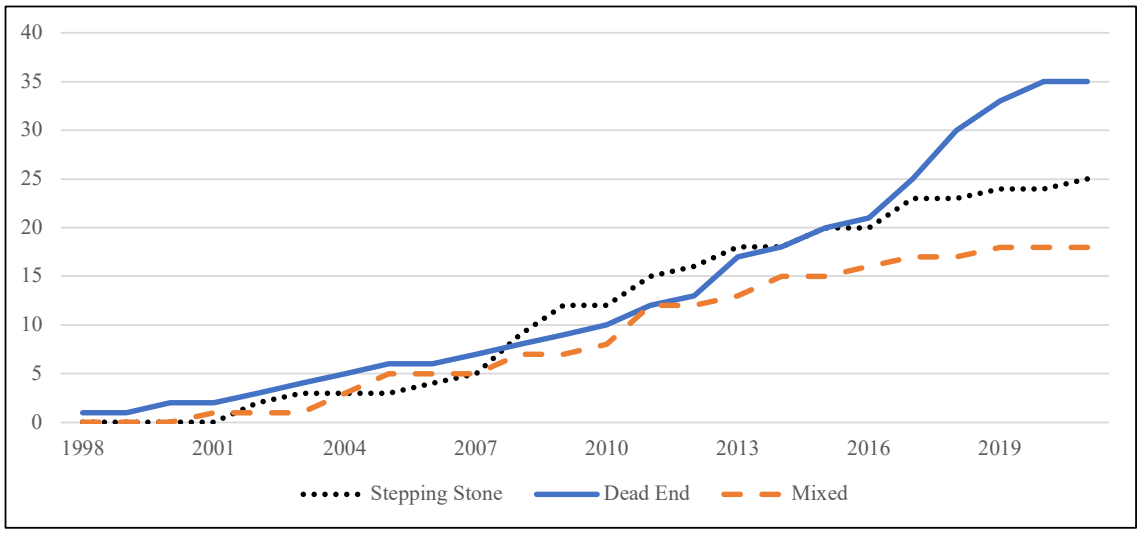
	Stepping Stone	Dead End	Mixed/Controversial	Total
1990-1999	0	1	0	1
2000-2009	12	8	7	27
2010-2020[(a)]	13	26	11	50

[(a)] In this time frame we also include the first 2 months of 2021.

4.2 Study outcome by research design

When investigating the impact of temporary jobs on subsequent labour market performances at individual level, analysts typically compare the careers of workers who experienced a temporary job, or repeated temporary positions, to the career of control units

Figure 2: Cumulative absolute frequency over years by research outcome



who were either not employed or had an open-ended job. In this framework, researchers are especially far away from randomization, the gold standard for causal inference. When contrasting labour market outcomes of temporary workers to those of other individuals, researchers face indeed the usual crux in an evaluation framework, i.e. the lack of the random assignment to the treatment and to the control groups: there might be confounding variables that are able to determine both the labour market outcomes of interest (e.g. probability of having a stable job or earnings in some years) and the probability of being in a particular labour market state (e.g. non-employed, temporary job, or open-ended contract) when observed. Different approaches have been used to solve the endogenous selection into the labour market status and claim identification of the causal effect of having a temporary job on future labour market outcomes.

The different identification strategies used in the studies in our sample can be distinguished in two broad categories: methods based on *selection on observables* and those based on *selection on unobservables*. In what follows, we list and comment on a set of methodological identification strategies, among which the first two belong to the selection on observables approach and the others to the selection on unobservables:

1. *Control Function approach (CF)* introduces into the regression model all the observables that could possibly be correlated with the treatment variable and explain the outcome. We adopt this definition for those studies that introduce a considerable

number of controls into their regression analysis. The main limit of such an approach is that there might always be further time-constant and time-varying heterogeneity across individuals that the researchers cannot control for and leading to an omitted variables bias. In our sample, 6 studies apply this method in order to estimate the causal effect of temporary jobs on subsequent position, of which 3 support the dead end hypothesis.

2. *Propensity Score Matching (PSM)* reduces the differences between the treated and the control groups using the propensity score, an estimate of the probability of receiving the treatment (Rosenbaum and Rubin, 1983) based on a large set of individual characteristics. Similarly to the previous approach, the underlying main assumption to identify the causal effect of the treatment is that there should not be systematic differences between the two groups in unobserved characteristics determining the outcome and all the variables affecting simultaneously the outcome and the treatment are observed. From the operational viewpoint this approach is semi-parametric: the researcher first estimates the propensity score of each individual receiving the treatment using standard binary index models; the propensity score is then exploited to compare the outcome variable of each treated individual with the outcome variable of control units who are similar in terms of propensity score and who are used therefore as counterfactuals. 11 articles in our sample apply this method, and 5 of them find evidence supporting the stepping stone hypothesis.

3. *Instrumental Variables (IV)* rely on finding (at least) an additional variable, the instrumental variable, which is correlated to the treatment variable but orthogonal to the outcome. This procedure allows researchers to isolate the exogenous variation in the treatment to get unbiased estimates of the causal relationship between the outcome and the predictor. In practice, the treatment effect is usually estimated by the two-stage least squares (2SLS) estimator. The first stage consists in regressing the treatment variable on the instrument(s) and the other controls which are regressors in the main equation. In the second stage, the main equation is estimated by ordinary least squares after replacing the original treatment variable with the treatment prediction estimated in the first stage. Exogenous sources of variation meeting the assumptions for the IV validity are very difficult to find. Indeed, only 1 article in our sample uses IV as identification strategy and finds that temporary jobs are dead ends.

4. *Difference-in-Differences (DiD)* is used to estimate the effect of a specific intervention by comparing the changes in outcomes over time between treated and untreated units. If one group is exogenously exposed to a treatment or policy shift and the other is not, then the effect of the treatment can be easily measured taking the differences between the average results for the two groups, before and after the intervention. Subsequently, the impact or causal effect of the treatment is calculated as the difference between those two differences. The key assumption required to identify the effect of the treatment is that the trends in the outcome variable must be identical in both groups in the absence of the treatment. In several studies, the availability of longitudinal data on those who have not experienced an interruption in their job career allows to construct a control group and to estimate the effect for those who suffered it, against the control group (Arulampalam et al., 2001). This estimation is the *within-group estimation in fixed-effects panel regressions (FE)*, where the individual time-constant effect captures the unobserved heterogeneity that is possibly correlated with the treatment and the outcome and is removed by subtracting the within-individual time-averaged model from the original one. In our framework, the main limit of this identification strategy is that, albeit it controls for individual fixed effects, selection into treatment could be still endogenously induced by time-varying unobserved heterogeneity. We count 5 selected papers which adopt this identification strategy, of which only 1 supports the stepping stone hypothesis.

5. *Regression Discontinuity Design (RDD)* can be applied in specific settings when the probability of treatment participation changes discontinuously when a certain cut-off of a running (or forcing) variable is reached. The discontinuity in the outcome that is eventually observed at the cut-off can then be interpreted as the causal effect of the treatment in the neighbourhood of the cut-off of the running variable. The most common problem of RDD is that the treatment effect is identified only locally and it is not easily generalizable to the full sample (Cameron and Trivedi, 2005; Lee and Lemieux, 2010). In our sample, only García-Pérez et al. (2019) uses RDD and find evidence in favour of the dead end hypothesis.

6. *Timing of Events (ToE)* aims to assess how the instantaneous probability of finding a stable job is affected by starting a treatment spell. In this approach it is possible to remove from the estimated treatment effect the spurious component due to endogenous selection into treatment without using exclusion restrictions, as instead it is

done in IV approaches. The extra information in duration data that allows the identification of the treatment effect is the timing of events (Abbring and Van den Berg, 2003), which allows to identify the unobserved heterogeneity distribution. However, ToE has the drawback that treatment effect identification is based on a particular parametric specification of the hazard rates towards the treatment and towards the outcome state (mixed proportional hazard). This strategy is one of the most used in our sample: we count 11 articles, 6 of which support the stepping stone hypothesis.

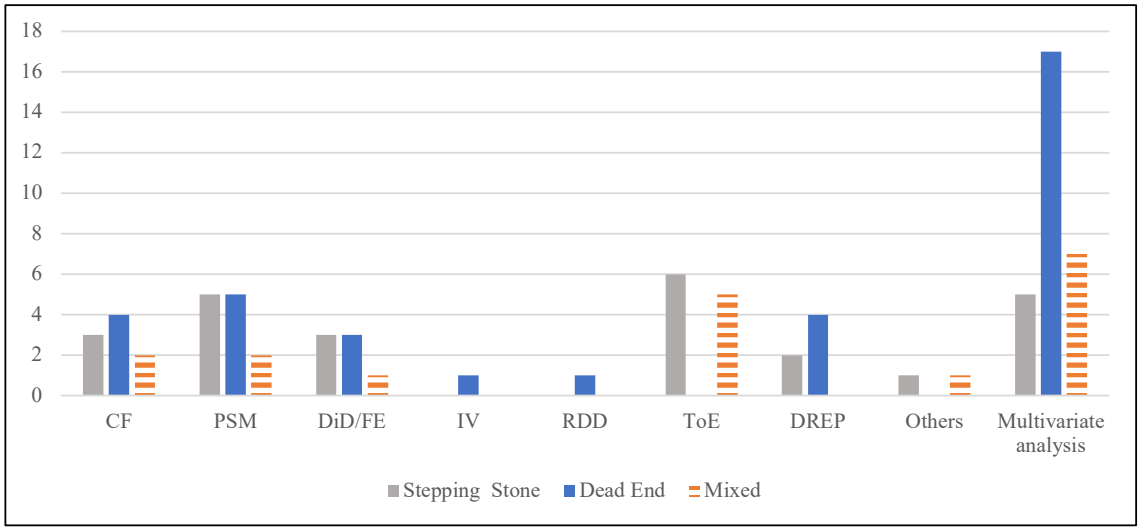
7. *Dynamic Random Effects Probit models (DREP)*. In these models, being non-linear, the fixed effects cannot be easily eliminated because of the incidental parameters problem. Hence, the the unobserved heterogeneity is treated as randomly distributed in the population and parametric approximations are used to take into account the correlation between the random term and the covariates (Mundlak, 1978; Chamberlain, 1984; Wooldridge, 2005). Among 6 articles which use this approach, 4 provide evidence in favour of the dead end hypothesis.

8. *Other methods*. Possible further approaches are based on: *cohort differences* under the assumption that the unobserved heterogeneity is constant across a number of cohorts; *structural dynamic models* which rely on models where the structure of decision making is fully incorporated in the specification of the model, describing the preferences and constraints of the process in order to identify the structural parameters; *hierarchical models* which take into account the nested nature of the data and thus control for the correlation between outcomes within each cluster structure. We assign 2 studies out of 64 to this category.

There are some studies with lack of focus on the issue of identification of the causal effect, i.e. multivariate descriptive analysis or traditional regression models (linear models, proportional hazard models, multinomial logit models) with a reduced number of controls. They apply none of the previous methods and no other methodology credibly designed to identify a causal effect in a non-randomized framework. Although we consider their results as weak in terms of causal interpretation, we keep them in our meta-analytic sample. By doing so, we can investigate in more detail the importance of credibly dealing with causality for the kind of research findings. In our sample, there are 29 observations from 21 articles weakly addressing causality. Among them, only 5 support the stepping stone hypothesis, 7 provide mixed results, and 17 find evidence in favour of the dead end hypothesis.

Figure 3 displays the absolute frequency of observations by research outcome and by the methodology used for the identification of the causal effect. The stepping stone hypothesis is prevalent in only one of the applied methods: ToE. The dead end hypothesis is instead by far the most common result in articles not well equipped in terms of identification strategy of the causal effect: out of 35 observations finding that temporary jobs are dead ends, 17 are from articles performing a simple multivariate analysis controlling only for a reduced number of regressors. If we removed from our sample articles not dealing properly with the self-selection into temporary positions, articles supporting the stepping stone hypothesis would become the most numerous: 40% against 37% of studies in favour of the dead end hypothesis and 23% providing mixed or controversial results.

Figure 3: Research outcomes by identification strategy

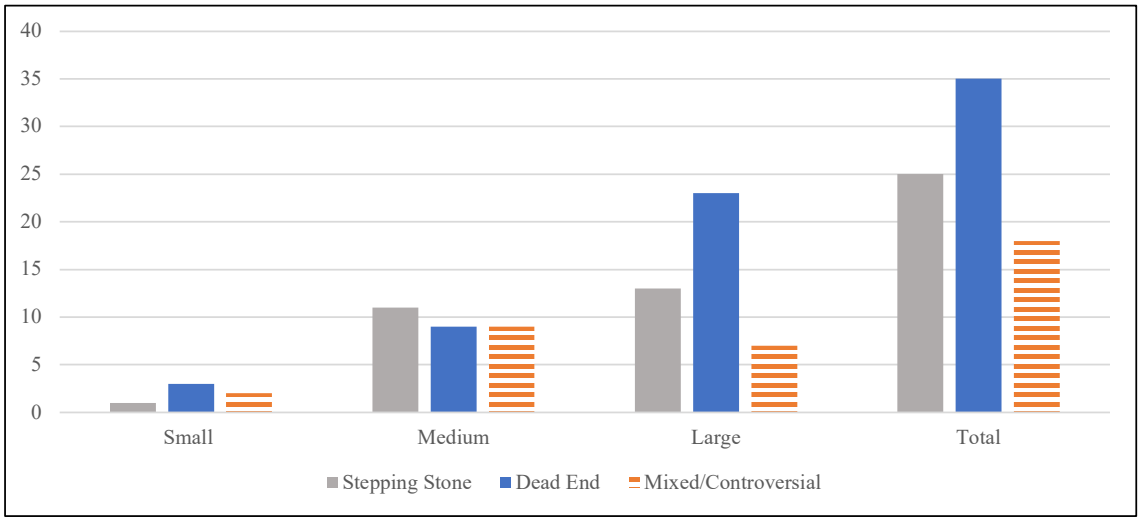


Notes: CF = Control Function; PSM = Propensity Score Matching; DiD = Difference-in-Differences; FE = Fixed-Effects ; IV = Instrumental Variables; RDD = Regression Discontinuity Design; ToE = Timing of Events; DREP = Dynamic Random Effects; Others = Other methods to credibly identify the causal effect; Multivariate analysis = multivariate descriptive analysis or traditional regression models with a reduced number of controls.

In the meta-analysis we will distinguish articles by the sample size used in the empirical analysis. We split articles in three groups: i) small sample size, i.e. less than 1,000 observations ($N < 1,000$); ii) medium sample size, i.e. between 1,000 and 10,000 observations ($1,000 \leq N < 10,000$); iii) large sample size, i.e. larger than 10,000 observations ($N \geq 10,000$). In our sample, less than 10% are small sample studies, 37% are medium sample studies and 55% have large samples. Figure 4 reports the research

outcomes by sample size: results from studies with larger samples are more likely to find support in favour of the dead end hypothesis (23 studies out of 43), while the support for the stepping stone hypothesis is relatively more likely in empirical analyses with medium sample sizes.

Figure 4: Research outcomes by sample size

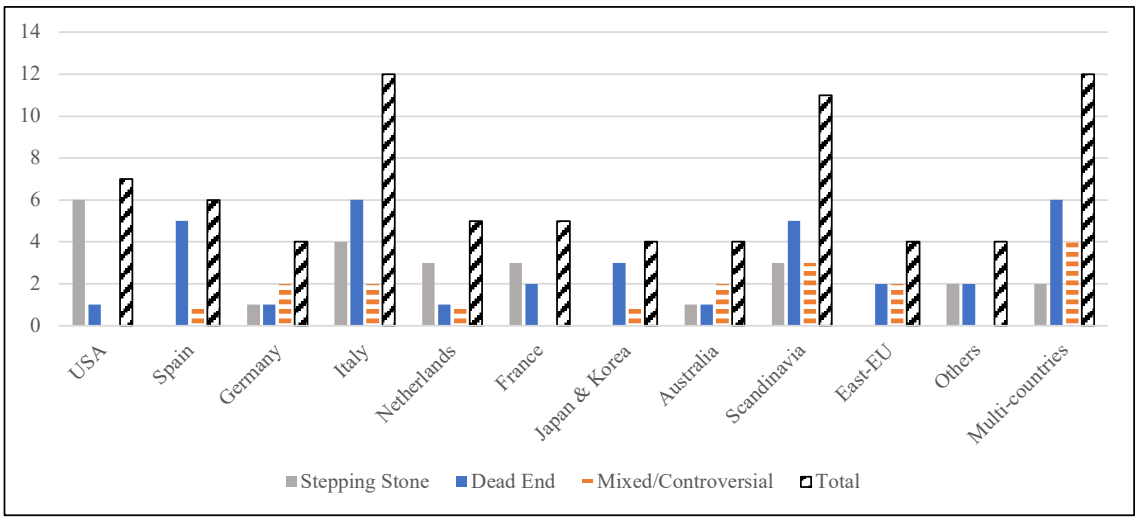


Notes: Small means $N < 1,000$. Medium means $1,000 \leq N < 10,000$. Large means $N \geq 10,000$.

4.3 Study outcome by countries, institutional context, and macroeconomic conditions

Figure 5 displays the absolute frequency of studies by countries and research outcome. Most of the articles focus on Italy and Spain, two countries where in the last three decades the introduction of temporary employment has been very important and created a strong labour market duality. The case of Spain is interesting as 5 out of 6 articles found support for the dead end hypothesis and none found evidence of the stepping stone effect. Also for Japan and South Korea there is no evidence for the stepping stone effect. On the opposite side, for the US 86% of the studies revealed that temporary jobs acted as port of entry into employment. The findings for the Netherlands are similar. In Italy, Germany, France, Australia, and the Scandinavian countries the results are more balanced and there is no clear evidence for a dominant research outcome.

Figure 5: Research outcomes by country



Notes: Scandinavia includes Denmark (2 articles) and Sweden (3 articles). East-EU includes Poland (2 articles) and Slovakia (1 article).

An explanation for the heterogeneous distribution of the research outcomes across countries could be provided by different labour market institutions. Institutional labour market regulations are indeed likely to be important in explaining the role of temporary jobs in the economy. In countries where permanent workers are very protected and their firing costs are high, temporary jobs could represent a port of entry for the most disadvantaged categories of workers such as women, the youth, and the long-term unemployed, who otherwise would not have the opportunity to prove their productivity. However, at the same time, the stricter the EPL for permanent workers, the more employers use temporary contracts as a buffer to face the business cycle and temporary jobs are more likely to be dead ends. Following Bentolila et al. (2019), employers' decisions heavily depend on firing costs and this favours dead end outcomes, in particular the larger the EPL gap between contract types: the higher the EPL gap, the larger the benefits of hiring using temporary contracts and the lower their conversion rate into permanent jobs.

Table 4 reports descriptive statistics of the EPL index of permanent employment, the EPL gap between temporary and permanent contracts, the unemployment rates, and the GDP growth rate by research outcome. These aggregate measures refer to the country studied in each article and to the year (or the midpoint of the time window) of the dataset

used for the empirical analysis. The number of observations is now reduced to 66 from 54 articles, since we excluded 10 articles using multiple countries. The EPL for permanent contracts is stricter in those countries where the research findings are in favour of the dead end hypothesis. However, when the gap between the EPL of permanent jobs and the EPL of temporary jobs is larger, the stepping stone hypothesis is more likely to be supported. In line with theoretical arguments, the stepping stone hypothesis is more likely to emerge when the state of the economy and of the labour market is more favourable, i.e. when the GDP growth rate is larger and the unemployment rate is lower.

4.4 Heterogeneity among different types of temporary jobs

The vast majority of the papers in our sample analyzed a single type of temporary contract, whereas 8 out of 64 reported results and conclusions on multiple contractual arrangements. Booth et al. (2002) report findings on fixed-term contracts (FTCs) and seasonal/casual jobs; Scherer (2004) uses both FTCs and very short employment episodes; Addison and Surfield (2009) find support for the stepping stone hypothesis for temporary, on-call and contracting jobs. Nunziata and Staffolani (2007) and Givord and Wilner (2015) provide evidence that FTCs are stepping stones but temporary help agencies jobs are dead ends. Berglund et al. (2017) distinguish among 7 contract types (substitute, probationary contracts, temporary jobs, seasonal, summer work, project and on-call jobs). Finally, Bosco and Valeriani (2018) and Kiersztyn (2020) find support for the dead end hypothesis for both FTCs and "parasubordinate" contracts and FTCs and irregular works, respectively. Here we investigate even whether and how the study outcome is associated to the type of temporary contract.[7]

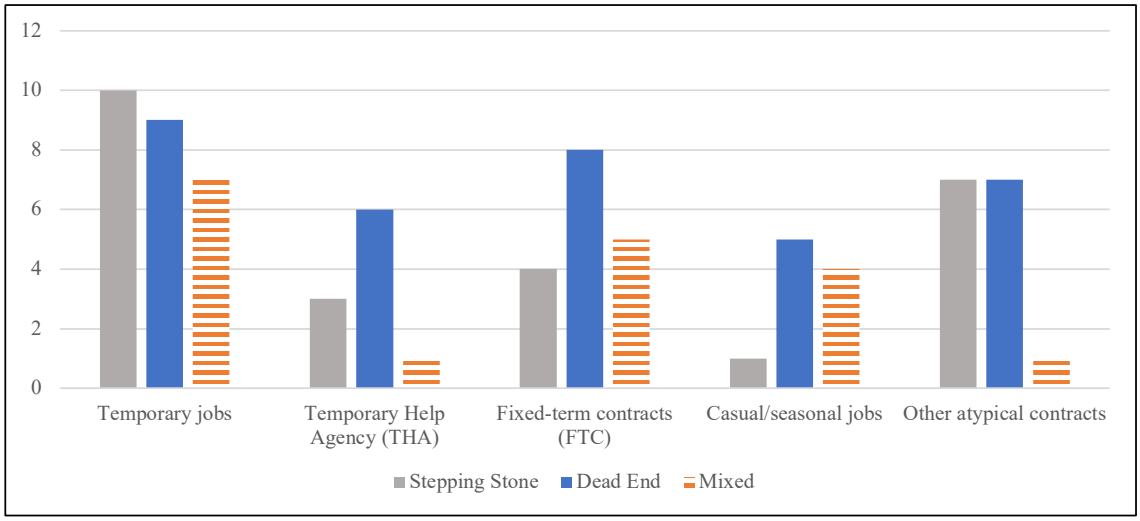
More in detail, we group temporary jobs in five categories. Figure 6 illustrates the research outcomes among these 5 categories. Firstly, 26 selected papers study the effect of temporary jobs without being specific in defining the contractual forms analyzed. We assign these papers to the generic category "Temporary jobs".[8] The stepping stone out-

[7]Further heterogeneity dimensions that could be interesting to investigate are those related to the probability of individuals to belong to disadvantaged groups, for example gender, labour market experience, education, age or blue vs. white collar workers. However, the studies in our sample rarely report separate estimates for these different groups of workers or, if they do, the results do not differ across the distinguished categories. For instance, although about 20 studies report separate estimates for men and women, the effect of temporary jobs on future job stability is similar for men and women and only in few cases the conclusions are different.

[8]This category includes those studies which do not distinguish among different forms of temporary con-

come slightly prevails in this category with 10 articles supporting it. Secondly, 10 papers analysed temporary help agencies (THA) jobs and the dead end effect is the most likely research outcome, while 17 studies focus on fixed-term contracts (FTCs) and, also in this case, the dead end effect is the most likely outcome. A further category includes seasonal contracts and all atypical contracts that are contingent, casual, or marginal, typically with limited job security and part-time. Again, the prevailing outcome is the dead end effect. Finally, a residual category captures all the other contractual forms, such as "parasubordinate" workers,[9] on-call jobs, subsidized temporary contracts, project works, summer works, short-lived jobs, and single quarter jobs. In this case, the two opposite hypotheses are equally supported by the empirical evidence.

Figure 6: Research outcomes by contract type



Notes: The category "Temporary jobs" includes those studies which do not distinguish among different forms of temporary contracts and studies which have "temporary job" as a separate category from other temporary arrangements. It also includes those studies using the definition of "atypical" or "non-standard work". The category "Casual/seasonal jobs" refers to seasonal contracts and all atypical contracts that are contingent, casual, or marginal. The category "Other atypical contracts" refers to those studies which explicitly use a particular type of temporary contract different from THA jobs and FTCs.

tracts and studies which have "temporary job" as a separate category from other temporary arrangements. This category also includes those studies using the definition of "atypical" or "non-standard work".

[9]"Parasubordinate" workers are officially self-employed workers, but *de facto* are employees, much used in Italy in the 2000s and 2010s.

Table 4: Descriptive statistics of institutional context and macroeconomic conditions by study outcome

	Stepping Stone	Dead End	Mixed/Controversial
a) EPL of permanent jobs			
Mean	1.989	2.409	2.376
Standard deviation	1.303	0.684	0.672
Minimum	0.090	0.090	1.230
Maximum	3.620	3.350	3.320
b) EPL gap			
Mean	0.378	0.427	0.422
Standard deviation	0.864	0.803	1.113
Minimum	-0.790	-0.890	-1.740
Maximum	2.360	2.050	2.180
c) Unemployment rate			
Mean	7.243	8.938	8.871
Standard deviation	1.869	3.989	3.153
Minimum	3.700	4.000	4.800
Maximum	10.300	20.400	16.800
d) GDP growth			
Mean	2.539	1.947	2.550
Standard deviation	0.842	1.574	1.322
Minimum	1.220	-2.300	0.100
Maximum	4.200	4.600	5.100
Observations	23	29	14

Source: OECD Database. The EPL of permanent jobs is the "strictness of employment protection: individual and collective dismissals (regular contracts)". We calculate the EPL gap as the difference between the EPL of permanent jobs and the EPL of temporary jobs. The unemployment rate comes from the General statistics, Key short-term economics indicators. The GDP growth comes from the Annual national accounts, Main aggregates.

5 Meta-regression

5.1 Method

The outcome variable of our model is given by the hypothesis supported by each evaluation study and categorized in three ordered values, following the same approach as the one in Kluve (2010) and other meta-analysis concerning evaluation effects in the labour market (see e.g. Kluve and Schmidt, 2002). Our ordered response dependent variable takes three value: -1 when the study supports the dead end hypothesis; 0 for mixed or controversial results; 1 when the research outcome is in favour of the stepping stone hypothesis.

Although we are aware that a meta-analysis should take into account the effect size and standard error of each study to provide solid evidence on eventual publication bias and on the genuine effect, we cannot have a finer approach for several reasons. First, the labour market outcomes used as dependent variables are very different across studies, going from the probability of having a stable jobs in different years in the future, to hazard rates towards permanent positions, to the probability of starting a job that lasts long enough, to earnings. Second, different identification strategies are followed and different counterfactuals are used. For example, in some studies the stepping stone hypothesis is investigated by comparing the future labour market outcomes of individuals who had a temporary job with those of individuals who had in the same moment a permanent position. In other articles, the same comparison is carried out between temporary workers and the unemployed (or not employed). Third, some studies focus on specific types of temporary employment, like temporary work agency jobs or fixed-term jobs, some other studies distinguish between various forms of temporary employment, and some others build their conclusions on eventual repetitions of temporary jobs (e.g. Gagliarducci, 2005; Sanz, 2011). Finally, while some studies focused on a single outcome variable, others based their conclusions on more than one outcome, for example on both subsequent employment stability and earnings.

For these reasons, we cannot use the MAER-NET guidelines (Stanley et al., 2013; Havránek et al., 2020) and opt instead for a simple ordered response variable taking on three values. A further consequent limit of our meta-analysis is the inability to seriously analyze the issue of publication bias, i.e. the bias stemming from the tendency of editors to publish more easily results consistent with a conventional view (Card and Krueger,

1995). This type of bias is usually investigated through a funnel plot, a scatter diagram of precision vs. non-standardized effect using the inverse of the standard error as a precision measure, or through a funnel asymmetry test (Stanley, 2005). We can only provide some suggestive evidence for the publication bias. However, we consider the applied methodology useful for conducting a meta-analysis on a very heterogeneous sample of articles, such as that of our study and, more in general, whenever it is particularly difficult to identify the main impact estimate or a standardized measure for the effect size.

Since the dependent variables is an ordered response variable, we model the probability that each study i is assigned to one of the three categories as a function of a set of covariates using the ordered probit specification:

$$\begin{aligned} P(y_i = -1|\mathbf{x}_i) \equiv p_{-1}(\mathbf{x}_i) &= \Phi(\alpha_0 - \mathbf{x}_i\boldsymbol{\beta}), \\ P(y_i = 0|\mathbf{x}_i) \equiv p_0(\mathbf{x}_i) &= \Phi(\alpha_1 - \mathbf{x}_i\boldsymbol{\beta}) - \Phi(\alpha_0 - \mathbf{x}_i\boldsymbol{\beta}), \\ P(y_i = 1|\mathbf{x}_i) \equiv p_1(\mathbf{x}_i) &= 1 - \Phi(\alpha_1 - \mathbf{x}_i\boldsymbol{\beta}), \end{aligned} \tag{1}$$

where α_0 and α_1 are cut points, $\mathbf{x}_i$ is a set of covariates, and $\boldsymbol{\beta}$ the conformable vector of parameters. Among the covariates we include the SJR indicator at the time of publication, the average number of citations per year according to Google Scholar and retrieved on 8 March 2021, the subject area, the year of publication, the sample size, dummies for the identification strategy, and indicators for the geographical area. Moreover, we include in the model specification dummy variables capturing different forms of temporary contracts. We use the average number of Google Scholar citations, albeit it is realized after the publication of the study, as a proxy for the quality and scientific credibility of the article, similar to Brancaccio et al. (2020). We implicitly assume therefore that there is no citation bias in this issue, i.e. we suppose that the probability of being cited is, *ceteris paribus*, independent on the study outcome (Jannot et al., 2013; Urlings et al., 2021).

The order probit model is estimated by maximum likelihood. Then, to quantify the correlation between the covariates and the response probabilities, we use the estimated parameters to calculate the average marginal effects of the different regressors on $p_{-1}(\mathbf{x}_i)$ and $p_1(\mathbf{x}_i)$, i.e. on the probability that a study finds support for the dead end hypothesis or the stepping stone hypothesis, respectively. By doing so we can quantify the impact of each characteristics by keeping fixed all the remaining ones.

5.2 Main results

Table 5 reports the full set of estimation results of the ordered probit model with three different sets of control variables. Specification (1) is the most parsimonious one: we include only two dummies for the identification strategy (selection on observables and selection on unobservables, with studies controlling only for a small set of covariates as the reference) and dummy variables for the type of contractual arrangements. We do not use the variables capturing the macroeconomic condition and the institutional context. In specification (2) we include a richer set of dummies for the identification strategy used to estimated the causal effect of temporary employment. Finally, in specification (3) we augment the baseline specification with the variables for the macroeconomic condition and the institutional context. For these 3 specifications, Table 6 displays the average partial effects of each regressor on the probability that a study finds support for the dead end hypothesis (APE_{-1}) and reports evidence in favour of the stepping stone hypothesis (APE_1).

The results in specification (1) of Tables 5 and 6 indicate that the quality of the publication as measured by the number of citations per year is strongly associated to the study outcome. One more citation per year is associated with an increase in the probability that the stepping stone is the study outcome by 0.9 percentage points (pp) and with a similar decrease in the probability of finding that temporary jobs are dead ends. This suggests that articles providing evidence supporting the stepping stone hypothesis receive more citations. Otherwise, the SJR index of the journal seems to be not significant.

The year of publication matters. We control for year of publication after grouping it in 3 categories. We find that papers published more recently are less likely to find results supporting the stepping stone hypothesis. We also observed this in the univariate analysis in Section 4 and it could be due to the fact that more unstable and stagnant economic conditions, as those which followed the Great Recession, largely favoured the use of temporary employment as a buffer.

The subject area of the journal is strongly associated to the study outcome. Articles published in multi area journals according to the Scimago classification are more likely to report evidence in favour of the stepping stone hypothesis and less likely to find that temporary jobs are dead ends. Observations from articles published in Economics, Econometrics and Finance or in Business, Management and Accounting journals are more likely to support the dead end scenario and less likely to present findings in favour of the

Table 5: Full set of estimation results of the ordered probit model for the study outcome

	(1)			(2)			(3)		
	Coeff.		Std.Err.	Coeff.		Std.Err.	Coeff.		Std.Err.
Google scholar citation per year	0.035	***	0.012	0.042	***	0.014	0.018		0.014
SJR index	-0.281		0.212	-0.274		0.223	-0.070		0.265
Year of publication - Reference: 2015-2021									
1999-2009	0.685	*	0.366	0.253		0.429	1.004	*	0.547
2010-2014	0.906	**	0.384	0.712	*	0.408	1.211	***	0.456
Subject area - Reference: Multi area									
Economics, Econometrics and Finance	-1.192	***	0.387	-1.653	***	0.511	-1.801	***	0.459
Social sciences	-0.116		0.449	-0.181		0.457	-0.853		0.645
Business, Management and Accounting	-1.636	***	0.531	-1.444	***	0.519	-1.569	***	0.514
Sample size - Reference: $N < 1,000$									
$1,000 \leq N < 10,000$	1.184	*	0.692	1.709	**	0.794	1.943	**	0.798
$N \geq 10,000$	1.364	**	0.683	1.878	**	0.790	1.703	**	0.735
Continental dummies - Reference: UE									
Extra EU	1.428	***	0.526	1.829	***	0.497	-0.600		0.963
Multi country	-0.577		0.391	-0.393		0.458	–		–
Contract type - Reference: Temporary jobs									
Fixed-term contracts	-0.186		0.372	0.042		0.420	-0.194		0.564
Temporary Help Agencies	-1.677	***	0.637	-1.740	***	0.648	-2.048	***	0.671
Casual/Seasonal jobs	-1.621	***	0.515	-1.664	***	0.531	-1.437	**	0.625
Other atypical contracts	-0.006		0.355	-0.097		0.367	0.302		0.500
Identification strategy - Reference: Multivariate analysis with reduced number of controls									
Selection on observables	1.365	***	0.457	–		–	1.715	***	0.509
Selection on unobservables	1.546	***	0.405	–		–	1.619	***	0.532
Identification strategy - Reference: Multivariate analysis with reduced number of controls									
Control function	–		–	1.800	**	0.771	–		–
Propensity score matching	–		–	1.637	***	0.519	–		–
DiD-FE	–		–	1.503	***	0.513	–		–
Timing-to-Events	–		–	2.554	***	0.771	–		–
Dynamic random effects probit	–		–	0.681		0.835	–		–
Other methods, including RDD and IV	–		–	0.808		0.869	–		–
Institutional context and macroeconomic conditions									
EPL for permanent workers	–		–	–		–	-0.873	*	0.483
EPL gap permanent/temporary workers	–		–	–		–	0.111		0.216
Unemployment rate	–		–	–		–	-0.128	**	0.062
GDP growth rate	–		–	–		–	0.268	*	0.142
Ordered probit cut points									
α_0	1.626		0.839	2.086		0.966	-0.625		1.726
α_1	2.504		0.858	3.028		0.992	0.311		1.708
Observations (studies)	78 (64)			78 (64)			66 (54)		
Pseudo-R^2	0.244			0.283			0.335		

Notes: *** Significant at 1%, ** significant at 5%, * significant at 10%. The outcome variable is an ordered response variable indicating whether a study supports the dead end hypothesis (-1), finds mixed or null results (0), or finds evidence in favour of the stepping stone hypothesis (1). The standard errors are clustered at study level.

Table 6: Average marginal effects from ordered probit model regressions

	(1)						(2)						(3)					
	$\frac{\partial P(y=-1\|\mathbf{x})}{\partial x_j}$			$\frac{\partial P(y=1\|\mathbf{x})}{\partial x_j}$			$\frac{\partial P(y=-1\|\mathbf{x})}{\partial x_j}$			$\frac{\partial P(y=1\|\mathbf{x})}{\partial x_j}$			$\frac{\partial P(y=-1\|\mathbf{x})}{\partial x_j}$			$\frac{\partial P(y=1\|\mathbf{x})}{\partial x_j}$		
	APE_{-1}		Std.Err.	APE_1		Std.Err.	APE_{-1}		Std.Err.	APE_1		Std.Err.	APE_{-1}		Std.Err.	APE_1		Std.Err.
Google scholar citations per year	-0.010	***	0.003	0.009	***	0.003	-0.011	***	0.003	0.010	***	0.003	-0.004		0.003	0.004		0.003
SJR index	0.076		0.056	-0.070		0.053	0.071		0.057	-0.062		0.052	0.016		0.061	-0.015		0.058
Year of publication - Reference: 2015-2021																		
1999-2009	-0.186	*	0.098	0.170	*	0.088	-0.066		0.111	0.057		0.097	-0.233	*	0.121	0.218	**	0.110
2010-2014	-0.246	**	0.100	0.225	**	0.093	-0.185	*	0.103	0.161	*	0.095	-0.281	***	0.098	0.263	***	0.098
Subject area - Reference: Multi area																		
Economics, Econometrics and Finance	0.324	***	0.088	-0.297	***	0.082	0.429	***	0.113	-0.373	***	0.100	0.417	***	0.088	-0.391	***	0.087
Social sciences	0.031		0.122	-0.029		0.111	0.047		0.118	-0.041		0.102	0.198		0.142	-0.185		0.130
Business, Management and Accounting	0.445	***	0.133	-0.407	***	0.124	0.375	***	0.125	-0.326	***	0.114	0.363	***	0.116	-0.341	***	0.110
Sample size: Reference: $N < 1,000$																		
$1,000 \leq N < 10,000$	-0.322	*	0.179	0.295	*	0.166	-0.443	**	0.189	0.386	**	0.165	-0.450	**	0.179	0.422	**	0.180
$N \geq 10,000$	-0.371	**	0.171	0.340	**	0.162	-0.487	***	0.187	0.424	**	0.169	-0.394	**	0.157	0.370	**	0.162
Continental dummies - Reference: UE																		
Extra EU	-0.388	***	0.125	0.355	***	0.119	-0.474	***	0.112	0.413	***	0.105	0.139		0.224	-0.130		0.209
Multi country	0.157		0.106	-0.144		0.096	0.102		0.117	-0.089		0.101	–		–	–		–
Contract type - Reference: Temporary jobs																		
Fixed-term contracts	0.050		0.101	-0.046		0.093	-0.011		0.109	0.009		0.095	0.045		0.132	-0.042		0.123
Temporary Help Agencies	0.456	***	0.154	-0.417	***	0.141	0.451	***	0.152	-0.392	***	0.134	0.475	***	0.150	-0.445	***	0.136
Casual/Seasonal jobs	0.440	***	0.117	-0.403	***	0.113	0.432	***	0.120	-0.375	***	0.108	0.333	**	0.131	-0.312	***	0.119
Other atypical contracts	0.002		0.097	-0.002		0.088	0.025		0.095	-0.022		0.082	-0.070		0.116	0.066		0.110
Identification strategy - Reference: Multivariate analysis with reduced number of controls																		
Selection on observables	-0.371	***	0.109	0.340	***	0.100	–		–	–		–	-0.397	***	0.110	0.373	***	0.114
Selection on unobservables	-0.420	***	0.089	0.385	***	0.089	–		–	–		–	-0.375	***	0.109	0.352	***	0.117
Identification strategy - Reference: Multivariate analysis with reduced number of controls																		
Control function	–		–	–		–	-0.467	**	0.187	0.406	**	0.161	–		–	–		–
Propensity score matching	–		–	–		–	-0.425	***	0.118	0.369	***	0.102	–		–	–		–
DiD-FE	–		–	–		–	-0.390	***	0.120	0.339	***	0.110	–		–	–		–
Timing-to-Events	–		–	–		–	-0.663	***	0.176	0.576	***	0.153	–		–	–		–
Dynamic random effects probit	–		–	–		–	-0.177		0.214	0.154		0.192	–		–	–		–
Other methods, including RDD and IV	–		–	–		–	-0.209		0.224	0.182		0.199	–		–	–		–
Institutional context and macroeconomic conditions																		
EPL for permanent workers	–		–	–		–	–		–	–		–	0.202	*	0.112	-0.190	*	0.101
EPL gap permanent/temporary workers	–		–	–		–	–		–	–		–	-0.026		0.050	0.024		0.047
Unemployment rate	–		–	–		–	–		–	–		–	0.030	**	0.013	-0.028	**	0.013
GDP growth rate	–		–	–		–	–		–	–		–	-0.062	*	0.033	0.058	*	0.032
Observations (studies)	78 (64)						78 (64)						66 (54)					

Notes: *** Significant at 1%, ** significant at 5%, * significant at 10%. The outcome variable is an ordered response variable indicating whether a study supports the dead end hypothesis (-1), finds mixed or null results (0), or finds evidence in favour of the stepping stone hypothesis (1). The average marginal effect of a continuous variable is estimated as the sample mean of the first partial derivative of the response probability of interest with respect to the continuous regressor, evaluated at the estimated parameters. The average marginal effect of a dummy indicator is estimated as the sample mean of the difference between the response probabilities of interest for the dummy being equal to one and the dummy equal to 0 (and evaluated at the estimated parameters). The standard errors are clustered at study level.

stepping stone hypothesis.

Studies with larger sample sizes and focusing on extra-EU countries are much more likely to find evidence for the stepping stone hypothesis.

We find that jobs for THA and causal/seasonal jobs are the most likely to be dead ends: according to specification (1), the probability that a study finds that the dead end hypothesis prevails is 46 (44) pp higher than the one of studies with a general focus on temporary employment. Similar magnitudes are obtained from specifications (2) and (3).

Finally, we find that papers dealing with the endogeneity of the temporary job treatment on the basis of either the selection on observables or the selection on unobservables are much more likely to have research outcomes in favour of the stepping stone hypothesis than those articles that do not tackle self-selectivity issues.

In model (2) we shed more light on the methodological aspect and provide a richer specification to describe better the identification strategy. We find that those studies using the ToE approach, and therefore exploiting the extra information provided by the timing with which the selection into treatment and the transition to the outcome state occur, are the most likely to find the stepping stone effect. Their probability of finding results compatible to the stepping stone hypothesis is 57 pp larger than that of articles not tackling self-selectivity issues (the reference category). They are followed by control function, PSM and DiD approaches, with a probability of finding results supporting the stepping stone hypothesis which is 34-41 pp larger than that of the reference category.

Finally, in specification (3) we include four extra regressors capturing the labour market regulations (the EPL for open-ended contracts and the EPL gap between permanent and temporary jobs), the state of the labour market (unemployment rate), and the business cycle (GDP growth rate). We lose 12 observations (out of 78), because 10 articles are multi-country studies. In this specification, the coefficient of the citations per year becomes insignificant, suggesting that the quality of the journals and the citations received by the articles are homogeneous across the study outcomes. We speculate that this might indicate that there is no publication bias. The same happens for the coefficient of the continental dummy "Extra EU". The remaining results reasonably match those of the baseline model. Further, we find that the variables for the EPL for permanent workers and the EPL gap do not correlate to the study outcome. Otherwise, the status of the labour market, as measured by the unemployment rate, is correlated to the study outcome, rather than to the GDP growth rate: the larger the unemployment rate, the lower (larger) the probability that the stepping stone (dead end) effect is the study outcome. This suggests that temporary

jobs are more likely to be used as a buffer to face the volatility of the demand of the final product when the economy is in a downturn and that the level of labour market deregulation, as measured by the EPL indexes, plays instead a limited role. This is in line with the findings in Brancaccio et al. (2020) who, in a meta-analysis on the macroeconomic impact of the labour market deregulation on employment and unemployment, found that the effect of the EPL index on employment and unemployment is on average nil.

5.3 Citation bias

To proxy the scientific quality of the studies, we included among the covariates the number of Google Scholar citations per year. However, this is a good proxy of the quality of the article if there is no citation bias. If there is citation bias, the study result would indeed determine, *ceteris paribus*, the number of citations per year. The latter would be in our model a regressor affected by reverse causality and would not be able to approximate the scientific reliability of the studies.

To assess whether this might be a problem we run two robustness checks. First, we re-estimate the model after excluding from the set of regressors the number of yearly citations. The estimation results are in Table 7. The main findings are confirmed: the stepping stone hypothesis is more likely supported by articles which based their identification strategy on the selection on unobservables, more in detail on the ToE and DiD, and when the unemployment rate is lower. We can only detect a minor difference in specification (3): the EPL for permanent workers is now significant (at 5%) in explaining the study outcome: the higher the EPL of permanent workers, the higher the probability that temporary jobs are dead ends. This is in line with the theory predicting that when firing costs of permanent workers are larger, firms are less likely to use temporary jobs as probationary period, bur rather as a buffer to face final product demand uncertainty.

Second, we test for the citation bias by linearly regressing the number of Google Scholar citations per year on dummy indicators for the study outcome and all the other control variables used in the baseline model. We do it twice: first, using the full sample; second, after removing the two top performing articles in terms of citations per year, i.e. Booth et al. (2002) and Ichino et al. (2008) published on the Economic Journal and Journal of Applied Econometrics, respectively. The distribution of the citations per year is highly skewed to the right, with the two top performing articles far away from the bulk of observations (see Figure 7) and the risk, therefore, that these two outlying observations

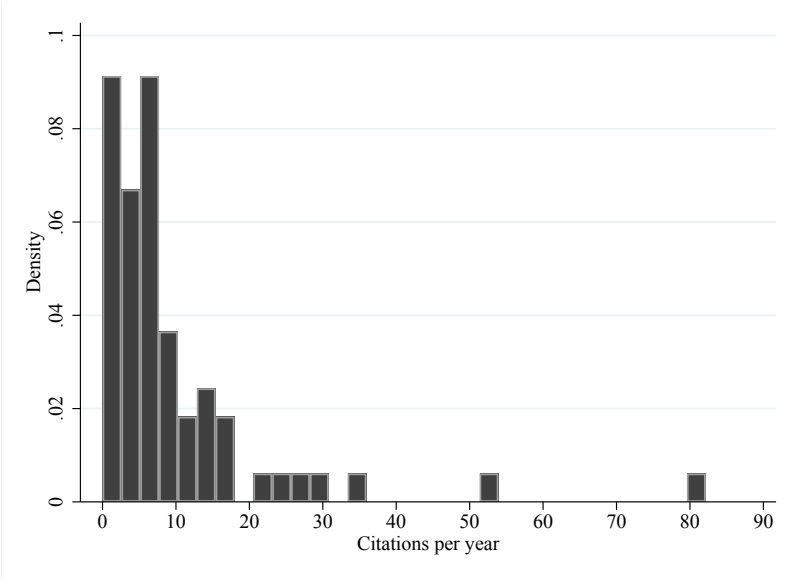
Table 7: Full set of estimation results of the ordered probit model for the study outcome without the number of citations per year

	(1)			(2)			(3)		
	Coeff.		Std.Err.	Coeff.		Std.Err.	Coeff.		Std.Err.
SJR index	-0.026		0.173	0.024		0.185	0.073		0.216
Year of publication - Reference: 2015-2021									
1999-2009	0.921	**	0.357	0.602		0.404	1.221	**	0.528
2010-2014	0.856	**	0.374	0.673		0.413	1.220	***	0.448
Subject area - Reference: Multi area									
Economics, Econometrics and Finance	-0.971	**	0.391	-1.203	**	0.514	-1.713	***	0.449
Social sciences	-0.302		0.418	-0.357		0.428	-1.016		0.629
Business, Management and Accounting	-1.492	***	0.513	-1.190	**	0.485	-1.469	***	0.522
Sample size: Reference: $N < 1,000$									
$1,000 \leq N < 10,000$	1.050		0.712	1.427	*	0.781	1.876	**	0.842
$N \geq 10,000$	0.978		0.645	1.267	*	0.707	1.444	**	0.677
Continental dummies - Reference: UE									
Extra EU	1.068	**	0.477	1.353	***	0.466	-0.992		0.801
Multi country	-0.592		0.414	-0.457		0.453	–		–
Contract type - Reference: Temporary jobs									
Fixed-term contracts	-0.112		0.375	0.036		0.374	-0.261		0.553
Temporary Help Agencies	-1.400	**	0.596	-1.526	**	0.625	-1.998	***	0.721
Casual/Seasonal jobs	-1.429	***	0.484	-1.519	***	0.538	-1.372	**	0.635
Other atypical contracts	0.056		0.367	-0.008		0.366	0.331		0.531
Identification strategy - Reference: Multivariate analysis with reduced number of controls									
Selection on observables	0.979	**	0.438	–		–	1.496	***	0.459
Selection on unobservables	1.201	***	0.373	–		–	1.433	***	0.477
Identification strategy - Reference: Multivariate analysis with reduced number of controls									
Control function	–		–	1.039		0.668	–		–
Propensity score matching	–		–	1.245	**	0.543	–		–
DiD-FE	–		–	1.241	***	0.462	–		–
Timing-to-Events	–		–	1.924	***	0.676	–		–
Dynamic random effects probit	–		–	0.284		0.802	–		–
Other methods, including RDD and IV	–		–	0.585		0.896	–		–
Institutional context and macroeconomic conditions									
EPL for permanent workers	–		–	–		–	-1.010	**	0.459
EPL gap permanent/temporary workers	–		–	–		–	0.128		0.221
Unemployment rate	–		–	–		–	-0.126	**	0.057
GDP growth rate	–		–	–		–	0.254	*	0.141
Ordered probit cut points									
α_0	1.203		0.794	1.450		0.863	-1.279		1.525
α_1	2.024		0.807	2.318		0.882	-0.363		1.496
Observations (studies)	78 (64)			78 (64)			66 (54)		
Pseudo-R^2	0.210			0.242			0.327		

Notes: *** Significant at 1%, ** significant at 5%, * significant at 10%. The standard errors are clustered at study level.

could generate an exceptional influence on the least squares line. Table 8 displays the ordinary least squares estimation results of the regression of the Google Scholar citations per year on the study outcome and the other control variables.

Figure 7: Distribution of Google Scholar citations per year of the 64 studies in our sample



In Model (1), which uses the whole sample, we find that the stepping stone outcome has a positive and sizeable effect on citations, albeit statistically significant only at the 10% level: studies finding support for the stepping stone hypothesis receive in a year on average 9 citations more than those which conclude that temporary jobs are dead ends. However, after excluding the top 2 performing articles in Model (2), which both found that temporary jobs are stepping stones, this association disappears. Booth et al. (2002) and Ichino et al. (2008) are two well known papers in this topic, published in journals with a tradition for being scientifically credible and reliable, and facing the issue using state-of-the art econometric techniques. The large number of citations they attract are likely due to the scientific relevance of their approach, rather than due to a citation bias. They can be therefore considered as outliers, when we looking for evidence of a citation bias. Once they are removed from the sample, we realize that there is no correlation between the study outcome and the subsequent citations, conditional on all the other controls. We interpret this as evidence for the absence of citation bias, which in turn supports the use of the number of citations per year as a proxy of the scientific quality of the article in the

Table 8: Ordinary least squares estimate of Google Scholar citations per year on study outcome and other controls

Dependent variable: Google Scholar citations per year	(1) Whole sample Coeff.		Std. Err.	(2) After removing top 2 articles in citations per year[(a)] Coeff.		Std. Err.
Study outcome - Reference: Dead end						
Mixed/Controversial	-1.191		3.090	-0.232		1.757
Stepping stone	8.776	*	4.784	0.390		1.954
SJR index	7.659	***	1.967	4.239	***	0.723
Year of publication - Reference: 2015-2021						
1999-2009	6.717	**	3.353	6.045	***	2.235
2010-2014	-0.378		3.166	3.319	*	1.879
Subject area - Reference: Multi area						
Economics, Econometrics and Finance	8.868		6.678	-0.844		1.681
Social sciences	-3.379		3.668	-0.684		2.423
Business, Management and Accounting	3.465		3.371	-0.084		2.168
Sample size: Reference: $N < 1,000$						
$1,000 \leq N < 10,000$	-8.722		7.306	-5.853		6.870
$N \geq 10,000$	-16.541		8.673	-5.216		6.609
Continental dummies - Reference: UE						
Extra EU	-14.170	**	6.113	-1.348		2.140
Multi country	0.042		3.360	3.880		2.534
Contract type - Reference: Temporary jobs						
Fixed-term contracts	-0.078		2.882	0.566		2.076
Temporary Help Agencies	9.105		5.493	-0.969		2.260
Casual/Seasonal jobs	11.112	*	6.506	-0.892		1.946
Other atypical contracts	0.225		3.393	2.108		2.228
Identification strategy - Reference: Multivariate analysis with reduced number of controls						
Selection on observables	-14.170	**	6.840	-5.964	**	2.887
Selection on unobservables	-12.979	**	6.241	-1.327	**	1.795
Constant	16.042	*	9.092	7.535		7.433
Observations (studies)	78 (64)			75 (62)		
R^2	0.594			0.544		

Notes: *** Significant at 1%, ** significant at 5%, * significant at 10%. The standard errors are clustered at study level.

[(a)] We removed the two top performing articles in terms of citations per year, Booth et al. (2002) and Ichino et al. (2008). Their realization is indeed far away from the bulk of observations (see Figure 7), with the risk that these two outlying observations could generate an exceptional influence on the least squares line.

baseline model.

6 Conclusions

This article presents a meta-analytical approach to the "stepping stone vs. dead end" debate concerning the effects of temporary jobs on subsequent labour market performances. To the best of our knowledge, this article is the first attempt to systematically and quantitatively summarize the empirical findings on this issue. We searched and collected 64 articles published on peer-review journals for the period 1990-2021, which provide an amount of 78 observations. Among these findings, 32% support the hypothesis according to which temporary contracts are a port of entry into stable jobs, 23% report mixed or no effects, and the remaining 45% provide evidence in favour of the dead end effect.

We analyzed how the study outcome is correlated to different study-related characteristics, the labour market regulation, the state of the labour market, and the business cycle. The meta-regressions suggested that the probability that the study outcome finds support for the stepping stone (dead end) hypothesis is larger (smaller) when the identification strategy of the causal effect of temporary employment relies on both selection on observables and unobservables, especially on the timing-of-events approach. By analysing whether the type of temporary contract matters, we found that THA jobs and casual/seasonal jobs are strongly associated with a higher (lower) probability of identifying a result in favour of the dead end (stepping stone) hypothesis.

Finally, we detected that the strictness of the EPL for permanent workers and its gap from the one for temporary workers are not associated to the study outcome. This evidence is in line with the results in the meta-analysis in Brancaccio et al. (2020), who found that labour market deregulation is not associated to better macroeconomic employment performances. Instead, our meta-analysis revealed that the unemployment rate plays a relevant role, suggesting that the stepping stone (dead end) effect is less (more) likely in economic downturns. A similar finding comes from the GDP growth rate, although with lower statistical significance.

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