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Comment

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The stated objective of this paper is to shed light on the forces that have shaped secular trends in the unemployment rate and the participation rate in the United States in recent decades. Due to space limitations, all of my comments will focus on the paper's analysis of the participation rate. Figure 1 in the paper shows that the participation rate has followed a pronounced inverted U-shape since the late 1960s, increasing by more than 7 percentage points between 1970 and the late 1990s, but subsequently decreasing by more than 4 percentage points.

The central message of the paper is that changes in the cross-sectional heterogeneity of *nonparticipants* are key to understanding the recent decline in the participation rate. To support this message, the authors use information from a question in the household survey that asks respondents whether they "want a job." Their key piece of evidence is that the fraction of non-participants who want a job displays a downward trend over the last three decades that accelerated during the later part of the 1990s.

While there are many details in the authors' analysis that merit discussion, my comments will focus on two broad points. First, I argue that the new labor market indicator proposed by the authors as a key (exogenous) driving force—the fraction of nonparticipants who want a job—suffers from serious endogeneity problems that the authors fail to recognize or deal with. Second, I suggest that there is a much simpler and preferable measure that the authors could have examined, namely, the fraction of the *population* that does not want a job. Analysis of this alternative measure yields a very clear negative relationship between it and the participation rate. But this finding—that the participation rate goes down when a greater fraction of the population does not want

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a job—seems little more than a tautology and does not strike me as a major insight into thinking about the underlying forces that shape secular movements in the participation rate. In summary, while I think the authors' focus on heterogeneity is appropriate and important, I am not convinced that the analysis has delivered new insights into the forces that have shaped the evolution of the participation rate.

To present my arguments, I need to first develop a theoretical framework. For purposes of exposition I develop the simplest framework that allows me to illustrate the key endogeneity problem that the paper ignores. This simple model will not display all of the salient features of transitions found in the data and as documented by the authors. While the key point I make is robust to extensions that would deliver these features, there would be a cost in terms of transparency.

The essence of the model is to provide a rationale for the existence of individuals who say that they want a job, but nonetheless do not engage in search. This sort of conceptual framework is clearly essential to interpret the authors' analysis. Let the population consist of a continuum of individuals, with total mass normalized to 1. These individuals are heterogeneous, with the heterogeneity summarized by a one-dimensional parameter, denoted by z, where z represents the net flow utility value associated with employment. A high positive z individual is someone who sees a very large flow benefit in working, while a very large negative z individual is someone who sees a large flow benefit in not working.[1] An individual's value of z is constant over time, and in what follows I will assume that the distribution of individuals across z has no mass points, so that we do not have to worry about how to assign individuals who are indifferent between two actions.

In a world without any labor market frictions, individuals with z positive would choose to work and individuals with z negative would choose not to work, and a labor market survey would not find any individuals not working who report wanting a job. But adding some basic labor market frictions and costly search can change this. In particular, assume that search is a discrete choice variable with the utility cost of search given by $\bar{u}$. If a nonemployed individual searches, they will receive an employment opportunity with probability ϕ_u. If a nonemployed individual does not incur the search cost, they may still receive an employment opportunity, but the probability is $\phi_n < \phi_u$. Also, assume that individuals who are employed in period t face a probability σ of losing their job at the beginning of the following period, in which case they find themselves nonemployed and need to decide whether to search.

In this frictional labor market, it remains true that anyone with z positive prefers to work, and anyone with z negative prefers not to work. Because individuals with z negative do not desire to work, they would clearly not exert search effort. But in the presence of costly search, individuals with sufficiently small positive values of z will also choose to not search, since the small positive benefit from working is not sufficient to overcome the utility cost associated with search. In fact, there will be a reservation value, denoted by $\hat{z}$, with the property that $\hat{z} > 0$ and only individuals with $z \geq \hat{z}$ choose to search.

At any point in time each individual in this economy is easily assigned to one of the three labor market states of employment (E), unemployment (U), and not in the labor force (N): nonemployed individuals with $z \geq \hat{z}$ will be counted as unemployed, while nonemployed individuals with $z \leq \hat{z}$ will be counted as not in the labor force. Moreover, and central to what follows, the model also delivers a group of nonparticipants that would answer yes to the question of do you want a job, namely those individuals with $0 \leq z \leq \hat{z}$.

Although this model features a continuous distribution of heterogeneity, in terms of labor market dynamics it is sufficient to categorize individuals according to which of the three types they are: those who accept employment when offered and search when not employed (i.e., $z \geq \hat{z}$), those who accept employment when offered but do not search when nonemployed (i.e., $z \in [0, \hat{z}]$), and those who do not accept employment when offered and do not search (i.e., $z \leq 0$). Consistent with the labels that the authors use in their paper I will label those with $z \in [0, \hat{z}]$ as marginal workers (M) and those with $z \leq 0$ as inactive workers (I). Those with $z \geq \hat{z}$ will be referred to as attached workers (A). Denote the shares of each of these three types in the population as μ_j, for $j = A$, M, or I. These fractions are, of course, functions of the cross-sectional density describing the heterogeneity in the economy. In a steady-state equilibrium, there will be a constant distribution of attached workers across the E and U states (determined by the values of ϕ_u and σ), a constant distribution of the marginal workers across the E and N states (determined by the values of ϕ_n and σ), and all of the inactive workers will be in the N state. For given values of ϕ_u, ϕ_n, and σ, the aggregate distribution of individuals across the three states E, U, and N will depend on the μ_j.

As noted above, this model is too simple to capture all of the patterns that the authors find in the data and emphasize in their analysis,[2] but it does capture the key mechanism linking participation and heterogene-

ity that the authors stress: if we move some individuals from the marginal group into the inactive group, then the (steady state) participation rate for this economy will fall. Importantly, the participation rate will also fall if we move some individuals from the attached group into the inactive group, so that the most basic result is simply that an increase in the size of the inactive group will decrease the participation rate. Given the structure that has been assumed, this result is a mathematical proposition and merely serves to establish the logical possibility that changes in composition can lead to changes in the aggregate participation rate. The key objective of this paper is to measure these compositional changes and assess their consequences; this is where I think the authors make a questionable choice about how to proceed.

The authors measure the fraction of *nonparticipants* who say they want a job and take this as a measure of the exogenous change in the composition of types, that is, they interpret a decrease in this ratio as evidence of a shift of individuals from *M* into *I*. The problem with this inference is a very basic one: in steady state, marginal individuals are distributed between *E* and *N*, with the relative size of the two groups dictated by the parameters ϕ_n and σ. It follows that the fraction of individuals in *N* who want a job is influenced not only by the μ_j but also by the ϕ_i and σ. Any attempt to isolate movements in composition would have to explicitly decompose the changes in this ratio into changes due to the two different sets of parameters. A basic limitation of the analysis in this paper is that the authors make no effort to do this.[3]

Moreover, it seems apparent that the failure to do so leads to some misguided inference on the part of the authors. It is well known that the 1990s was associated with a booming labor market and, in particular, very high job-finding rates. My simple model predicts that during such a period we will see a decrease in the fraction of nonparticipants who want a job, and as the authors note, this is exactly what is seen in the data. If this decrease were purely the result of a shift of individuals from either *A* or *M* into *I* then we would also have seen a drop in employment, but, of course, the 1990s saw a large increase in the employment-to-population ratio. Does this mean that *all* of the drop in the share of nonparticipants who want a job is due to changes in job-finding rates? Of course not, but the key point is that one cannot assume that all of the drop is driven by exogenous changes in composition, a point that the authors fail to adequately recognize or deal with.

However, while the authors adopt what I see as a seriously flawed empirical strategy, my model suggests a very simple alternative that is

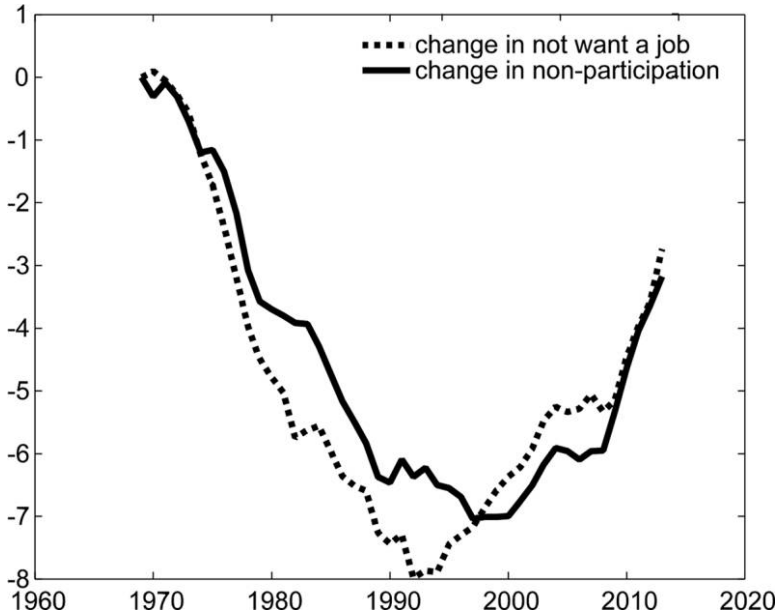

Fig. 1. Changes in share not wanting and job and nonparticipation

relatively immune to the basic endogeneity problem just described. The authors' approach was to look at the ratio of *M* to *I* within *N* to infer something about the overall movement of workers between *M* and *I*. The reason this was an unwise strategy is that the ratio of *M* individuals in *N* is itself affected by labor market conditions. As I noted earlier, there is nothing special about movements from *M* into *I* as opposed to movements from *A* into *I*. It follows that our primary concern should be to focus on the size of the *I* group. In my example one can directly measure the changing size of the *I* group by simply measuring the number of people in *N* who do not want a job, relative to the overall population.

What would the authors have found if they had instead focused on the size of the "don't want a job" group relative to overall population (i.e., the noninstitutionalized civilian population age 16 and older)? Using data supplied by the authors on their series for the share of nonparticipants who want a job along with BLS data on the civilian participation rate, I construct two series: the share of the population that does not want a job and the nonparticipation rate (i.e., the percentage of the population that is not in the labor force). Figure 1 plots the two series, normalized so that each is zero in the initial year, 1969.

The two series move almost perfectly in lockstep during both the increasing and decreasing phases of participation. However, whereas the turning point for the share of not want a job is in the early 1990s, the turning point for the participation rate is not until the late 1990s. The earlier discussion provides a likely rationale for this: the booming US economy in the 1990s was characterized by a high job-finding rate, which in turn lead to a greater share of M workers in the labor force, thereby exerting an opposing force on the participation rate.

The main message that comes from figure 1 is simple and mundane: understanding trend changes in participation is tantamount to understanding trend changes in the fraction of individuals who would like to work. This was true when participation was increasing during the 1970s and 1980s, and it remains true during the recent secular decline in the participation rate. Is this observation very powerful? I would say not. To be sure, for those who might think that the recent decline in participation is dominated by discouraged workers—those who want a job but have given up on searching—the answer might be yes, since in this case there could be a significant divergence of the two series. But we already know from numerous existing studies that changes in the size of the discouraged worker group is not driving the decrease in participation. Once this possibility is off the table, it seems to me that saying there is a decrease in the share of the population that wants a job is really just another way of saying that the participation rate is decreasing. That is, I think this observation is effectively tautological in nature and does not provide any powerful new insights into thinking about the forces that are shaping the decline in participation.

In the simple model that I described, changes in the share of the population in I reflect changes in the cross-sectional nature of heterogeneity as summarized by the distribution of z. In this sense I am fully in agreement with the authors that to understand changes in participation it is essential to understand the changes in the nature of cross-sectional heterogeneity. However, I see no basis for suggesting that the key changes in heterogeneity took place within the group of nonparticipants. It is entirely plausible that some individuals have moved from A into I, as would happen, for example, if some change caused an individual working full time to retire sooner than previously planned, or some change led a working mother to leave the labor force and care for her children.

But this distinction aside, the real issue is to isolate the underlying source of the changes in heterogeneity. In my simple model, the heterogeneity was summarized by the variable z. But this variable rep-

resents an aggregate of several underlying variables, including market conditions (e.g., the wage, or the price of child care), preferences toward work and/or nonmarket opportunities (e.g., attitudes toward market-provided childcare), and the policy environment (e.g., social insurance programs). It follows that the task is to find the dominant changes underlying the implied changes in the distribution of z. For example, if one looks at the literature on the rise of female labor force participation, it is understood that the proximate driving force is the increase in the share of women that want to work, and the whole point of the literature is to isolate the underlying causes of that increase.

The authors do make some attempt to tackle this issue, arguing that one-time changes in policy during the mid-1990s have played a key role. I think this discussion is interesting, and the possibility that some of the reforms had unintended consequences for participation is an intriguing one. But, at least in its current form, I do not yet find this analysis to be very compelling. The first and main reason is that all of their analysis is filtered through their measure of the share of nonparticipants who want a job, and as I have discussed previously, I see this as a very flawed measure for these purposes. Second, if one looks at figure 1, the share of the population in the "not want a job" category seems to display a very steady secular rise starting around 1992 and continuing until around 2005, at which point it levels off for a few years before showing another sharp uptick following the recent recession. At least on the surface, these dynamics would lead me to look for slow-moving driving forces rather than one-time changes, though this is obviously somewhat speculative.

Endnotes

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1. My analysis will treat z as exogenous. In a model in which individuals can save, the implicit flow value associated with work will be affected by endogenous savings decisions. But the key point I am making is robust to such an extension.

2. In particular, in this model individuals in M are never unemployed. One could either allow individual types to evolve stochastically, or assume that M individuals have a random component to z that causes them to switch between search and no search when nonemployed. Because I focus on implications for participation, I do not believe that these extensions are of first-order importance here.

3. Note that the value of $\hat{z}$ is in principle affected by changes in the ϕ_j and σ, so that my discussion is somewhat oversimplified.