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Does the Employment of Fewer Caseworkers Lead to the Rationing of Caseloads? Evidence from Public Assistance in Japan

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Abstract: While a number of empirical studies have explored the determining factors of welfare caseloads, none of them has examined the effect of workload on caseload. However, several studies outside the field of economics have suggested that workload may be an important factor in determining caseload size, in that higher workloads may lead to the rationing of assistance. This would mean that a greater number of caseloads per caseworker should decrease total caseloads. Using a panel of Japanese cities, this paper estimates the effect of workload on caseload size to examine whether the rationing of social assistance benefits does occur. The results support for the existence of the rationing. This study also examines the effects of caseload size on the number of caseworkers to see how localities adjust their caseworkers to increasing needs of social assistance. The estimation finds that the adjustment is quite sluggish. On average, the localities may well not even employ one additional caseworker, even if their caseload increases by almost 100.

Key Words: social assistance, workload, caseloads, caseworkers

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1. Introduction

A number of empirical studies have explored the various determinants of welfare caseload size. While there are some studies on this area from the 1960s (e.g., Brehm and Saving 1964), the majority of studies emerged in the 1990s in the US.[1] Most of the studies were prompted by the significant increase in caseload observed in the early 1990s, which was then followed by an abrupt decrease after 1994 accompanied by a concurrent series of welfare reforms at both state and federal levels. While almost all studies in the literature examine the effects of economic factors like unemployment on caseloads, many studies also explore the effects of institutional schemes, including state demonstration programs (Schiller and Brasher 1993; Johnson et al. 1994), waivers from Aid to Families with Dependent Children (AFDC) (Schiller 1999; Zillizk et al. 2000), the difference between AFDC-Basic and AFDC-UP (Blank 2001), child support enforcement (Huang et al. 2004), and the introduction of Temporary Assistance for Needy Families (Moffit 2003; Cadena et al. 2006; Danielson and Klerman 2008). Other studies highlights factors like benefits levels (Smith 1993; Shah and Smith 1995), at-risk populations (Conte et al. 1998), sluggish adjustments in caseload (Figlio and Ziliak 1999; Ziliak et al. 2000), discretion in implementation (Schiller 1999), within-state variation in the labor market (Lee et al. 2002; Lewis and Henry 2004; Hill and Murray 2008) and minimum wages (Page et al. 2005).

However, none of these US studies has examined the effect of workload on caseload, although several studies outside the field of economics suggest that workload may well play an important role in determining caseload size. Caseworkers with a heavy workload have little time to spend on work that goes beyond their job description (Ridzi and London 2006). In addition, as the number of caseloads increase, so also does the likelihood of high staff turnover in welfare agencies; this then results in the shuffling of assignments, which in turn intensifies the workload for those who remain (Smith 2005). Heavy workloads also reduce the efficacy with which caseworkers can conduct their casework, since, for example, it may increase caseworkers' vulnerability to burnout (Lloyd et al. 2002). This increases the likelihood that understaffed welfare agencies fail

[1] There are also studies on other countries. See Spindler and Gilbreath (1979) on Canada, Gustafsson (1984) on Sweden, Ayala and Pérez (2005) on Spain, and Suzuki and Zhou (2007) on Japan.

to offer the assistance that their clients are entitled to, by developing methods that ration benefits, yet do so obscurely (Lipsky 1984; Brodkin 1997). According to a survey that was conducted in three US cities by Moffitt (2003), for example, the most common reason for not applying for welfare was that the process was regarded as “too much hassle”; because of this, 34 percent of those who were entitled to some kind of benefit did not apply for that to which they were entitled. Many of the respondents reported being “discouraged” from applying or “treated badly” by caseworkers. Lens (2006) also documents that faulty procedures by TANF officials reduced the aid distributed to clients. Indeed, he found that the sanctions implemented by TANF caseworkers were, in fact, reversed nearly 50 percent of the time when clients appealed.

The argument above implies that higher workloads are likely to lead to the *rationing* of assistance. If this is the case, a decrease in the number of caseloads per caseworker should mitigate their workload and relax this rationing. In other words, more caseloads per caseworker will *decrease* total caseloads. However, the fact that very few studies have properly examined the effect of workload on caseload size means that this area has undergone very little analysis.[2] This paper thus examines the effect of workload on caseload size to substantiate the existence of the rationing of social assistance benefits, by improving on the previous studies on the following grounds.

First, the Japanese institutional setting provides advantages in examining the effects of workload on caseworkers. Since the assistance system follows uniform rules across the country, there are no regional differences in the system that need to be controlled for in the estimation. In addition, those responsible for implementing the programs (i.e., local governments) are unlikely to affect the policy parameters in the assistance system. This means that the endogeneity of benefits and other policy variables (Mayers 2000) is less of a concern in the Japanese context.

Second, I use data from a panel of cities in which programs are implemented rather than data aggregated at a higher level (prefectures) as the latter may result in an

[2] To the best of my knowledge, there is only such a study by Suzuki and Zhou (2007). In their analysis on Japan, they showed that bigger workloads tend to lead to larger caseloads, implicitly negating the existence of assistance rationing. They argue that an increase in caseworkers allows more staff to be involved in diverting claimants from reliance on welfare to work, thereby reducing the number of welfare recipients. However, since they estimated a static least squares dummy variable (LSDV) model with an aggregate panel of 47 Japanese prefectures, they did not allow for the issues that the US caseload literature has identified as to be seen below in the text.

aggregation bias (Grubb 1984). While there are US studies that have used data from an implementers' level (e.g., counties), they all examine only samples within a single state (Grubb 1984, Lee et al. 2002, Kerman and Haider 2004, Hill and Murray 2008). While it may indeed be possible to collect county-level data from different states to obtain a larger sample, the estimation would then need to take into account the institutional differences among states. On the other hand, because of the institutional uniformity across the country, this consideration is not needed for this study as mentioned above.

Third, I consider the dynamics of caseload changes. A static model is limiting as it ignores the possibility that caseload may sluggishly adjust to its determinants (Figlio and Ziliak 1999; Ziliak et al. 2000). Since caseload is a stock concept, its current value naturally has much in common with that of previous periods. Klerman and Haider (2004) show that if a factor affects the entries onto and exits from welfare, the effect on the caseload stock will be a distributed lag. At the same time, they caution that the lag is likely to last several years, and that a model with a once-lagged dependent variable should presume no duration dependence. Although studies have not empirically rejected duration dependence, the model I use here includes only a once- or twice-lagged dependent variable, mainly because the panel data I used is "short." Even so, such a dynamic model is still an improvement on the static model that many of the previous studies have utilized. Moreover, if a regression model is an approximation, parsimony might well be a virtue in itself.

Fourth, I allow for the endogeneity that Klerman and Haider (2004) caution against when conducting a dynamic panel analysis. There are two types of this kind of endogeneity in the current study. First, while the number of caseworkers is likely to be fixed in any given fiscal year in Japan, city management would change the number of caseworkers in the next period according to the past caseload trends, resulting in a feedback from the lags of the dependent variable (caseload) to an explanatory variable (workload). Second, while the dependent variable (caseload) in period $t-1$ is predetermined in period t, it is affected by that variable in period $t-2$ in a dynamic model. Predetermined variables with these types of feedbacks from a lagged dependent variable can be endogenous in a panel-estimation (e.g., Wooldridge 2002). Furthermore, there may be yet another endogeneity in the current study. If caseworkers ration assistance in the face of heavy workloads, it follows that endogeneity occurs due to the reverse causation (or simultaneity) from the dependent variable (caseload) to the explanatory variable (workload). This is because large caseloads imply heavy workloads when the adjustment of caseworker size to caseloads is sluggish. I allow for all these sorts of endogeneity by employing the GMM estimator by Arellano and Bond (1991).

Last, I examine the feedback from caseload size to the number of caseworkers; this is an area, which, to the best of my knowledge, no studies have examined so far. It is important that this effect be examined in the Japanese policy context. One of the policy claims by the Japanese Ministry of Health, Labour and Welfare (MHLW) is that local governments are not responding well to increasing caseloads. The MHLW then argues that understaffed welfare offices conduct cursory reviews of assistance applications that result in unnecessary social spending (Kimura 2006). In other words, caseworkers with heavy workloads are more likely to accept assistance applicants without the due assessments. By estimating the effect of caseload on the number of caseworkers, we should shed light on this policy claim.

The rest of the paper is organized as follows. Section 2 describes the Japanese system of social assistance. Section 3 presents the data and the regression models that to be used, and then explores the effect of workload on the size of caseloads. Section 4 explores the effect of caseload size on the number of caseworkers. Section 5 concludes.

2. Institutional background

In Japan, local governments implement Public Assistance (PA) through their welfare offices (*fukushi jimusho*), as is required by the PA Law in Japan.[3] There are two levels of government, with municipalities (cities, towns, villages, and Tokyo's special districts) as the first tier, and prefectures as the second tier. A national law (the Social Welfare Law) mandates cities (s*hi*), including Tokyo's special districts (*tokubestu-ku*) and prefectures (*ken*) to set up welfare offices to implement the PA programs. Towns (*cho*) and villages (*son*) are not required to do so by themselves and there are indeed a small number of towns that have their own welfare offices. For

[3] This paper defines "Public Assistance" as referring to a specific scheme for social assistance in Japan, called *Seikatsu Hogo* which literally means "the protection (*hogo*) of daily life (*seikatsu*)."

residents in towns and villages without their own welfare offices, prefectures ensure that their welfare offices to cover the population.

Welfare offices implement PA according to uniform national rules. First, all recipients are entitled to the minimum costs of living, although their eligibility is considered only after they make an application for the assistance. Second, the MHLW determines the minimum costs of living, and PA only fills the difference between the minimum costs and the income that an individual can earn with his/her best efforts. This means that the assistance is means-tested. Indeed, PA benefits are provided only after a careful examination of an applicant's financial situation. Third, benefits are paid on a household, not an individual, basis. The minimum costs of living account for household needs in such a way that they reflect different characteristics of household members, such as age, gender, mental and physical conditions, and various regional price levels. Note that, unlike the TANF programs in the US, the PA programs cover all types of population, including the elderly.

The local implementation of PA programs is monitored via a hierarchy of audits and supervision measures. First, welfare offices are staffed not only with caseworkers but also with supervisors. Second, the MHLW commissions prefectures to conduct audits of localities within their jurisdictions. Third, prefectures and large cities are themselves reviewed by audits that are directly conducted by the MHLW. Last, the managerial positions in the welfare-related sections of local governments are sometimes held by bureaucrats who are temporarily transferred from the central offices.

Despite this, the system also allows for the possibility of discretionary measures at the local level that fall outside of the national rules and central monitoring, especially when the intake officers interviews potential recipients and when caseworkers conduct means tests. Given that eligibility criteria are firmly set by the central government, local governments change PA caseloads solely by bureaucratic disentitlements. In particular, the way caseworkers conduct in-take interviews and means tests could function as potential sources of assistance rationing.

PA caseworkers in Japan are indeed facing heavy workloads. In FY2007, the average number of cases that each PA caseworker in cities was responsible for is 73.5 cases (where each case is a PA household). Furthermore, as Figure 1 shows, the average per-worker caseloads on a city basis vary from 12 to 354 cases, showing large regional discrepancies. Since many localities have been suffering from fiscal stringency over the last 20 years, if caseworkers are faced with heavy workload, there may be little external pressure to extend program benefits to all the eligible. As briefly discussed in the introduction, heavy workload is also expected to negatively impact the efficacy with which caseworkers can carry out their casework. For example, caseworkers may be likely to become vulnerable to burnout, which plausibly impairs their efficacy. Indeed, a study of 55 caseworkers at seven welfare offices in Japan shows that as many as 28 workers (51 percent) were found to experience “high burnout” (Takeda et al. 2002).

Figure 1

As discussed in the introduction, the above argument may imply that, if heavy workload is a cause of the rationing of PA benefits, an increase in the number of caseworkers will mitigate workload, relax implicit rationing, and increase total caseload. That means that higher workload would lead to larger caseloads. However, the opposite effect has also been claimed. When the MHLW negotiated the cost-sharing scheme for social assistance with local governments in 2005, it argued that caseworkers, in the face of increasing workload, would conduct cursory reviews, accepting more applications without due assessments (Kimura 2006). In addition, Suzuki and Zhou (2007) argued that the more caseworkers there are, the more members of staff there will be who are devoted to diverting beneficiaries from reliance on welfare to employment, thereby reducing welfare recipients. These imply that higher workloads lead to larger caseloads.

3. The effect of workload on caseload

3.1. Regression model

I first examine the effect of PA workload on PA caseload. Let y_{it} refer to the index of PA caseload size and x_{it} to the index of PA workload in locality i and year t (subscripts are defined analogously thereafter). Since most of the literature uses two indices for this index — the natural logarithm of caseload size (e.g., Huang et al. 2004) and the ratio of caseload size to population (e.g., Blank 2002) — I use these two types of dependent variables to examine the effects. Conceptually, caseload and workload are distinct constructs (Strolin et al. 2007). The term caseload is used here to refer to the total number of PA households that are assigned to all of the caseworkers in a locality.

The term workload, however, refers to the amount of time it takes for a single caseworker to complete his/her designated tasks; these tasks may include direct client contact, paper work, supervision, and interagency collaboration. Since it is difficult to directly measure workload according to this definition, I use caseloads per caseworkers within a locality as a surrogate variable for "workload" since caseloads per caseworkers should correlate strongly with individual caseworkers' workload.

Therefore, the regression model can be formulated as:

$$y_{it} = \alpha \cdot y_{it-1} + \beta \cdot x_{it-1} + \sum_k \gamma_k \cdot w_{k.it} + c_i + u_{it}, \tag{1a}$$

where w_{it}s are the controls, including time dummies; c_i is an unobserved heterogeneity; u_{it} is an idiosyncratic error; and the Greek letters are parameters to be estimated. The control variables $w_{k,it}$ are unemployment rate, the ratio of elderly people (≥ 65) to the total population, the natural logarithm of population, the natural logarithm of per capita taxable income, a measure of local fiscal climate, and time dummies. The estimation that follows regards these variables as strictly exogenous. These controls are selected mainly by their availability at the municipal level.

Although the number of these controls is limited, the unobserved heterogeneity c_i could potentially control a number of important factors. Some of these factors have been identified by previous studies as follows. First, Keiser and Soss (1998) argue that caseworkers may tend to adopt the collective values shared within their organizations, and that these administrative factors could have a major effect on policy implementation. Second, potential barriers to take-up of welfare may include the spatial accessibility of welfare offices. A sparsely populated locality would have to devote large resources to outreach potential recipients. In other words, in areas where the problems of welfare office access are severe, caseloads might be smaller. Grubb (1984) surrogates this accessibility with population density, which is controlled by taking into account surface area, since the model (1a) includes population. Third, community attitudes should also be regarded as an important factor (Grubb 1984; Weissert 1994), as such attitudes may discourage eligible individuals from applying for welfare, or cause caseworkers to take stricter positions on eligibility assessment. Note that the administrative values, surface area,[4] and community values are unlikely to change during a short period of time but

[4] Since the sample excludes localities that merged during the period of consideration, the surface

are likely to differ across localities. Therefore, the unobserved heterogeneity may be able to capture the effects of the three factors, as well as the other factors that differ across localities but are stable over time.

3.2. The sample and data

I obtained unpublished municipal data on PA caseloads from the MHLW and those for PA caseworkers from the Ministry of Internal Affairs and Communications (MIC). The caseload data are measured by taking an annual average of the daily PA recipients in a given fiscal year (FY), whereas the caseworker data are those at the beginning of a given FY. The panel data are taken from a period that began in FY1998 and ended in FY2003, and therefore the panel is "short." The opening year (FY1998) was chosen because it is the year for which the oldest data on the number of caseworkers are available on a municipal basis. The closing year (FY2003) was chosen because a large number of municipalities merged after FY2004.

All of the data that I included in the data sample came from cities. As I explained previously, cities and prefectures are mandated to implement PA through their welfare offices. Prefectural welfare offices cover residents in those towns and villages that are not required to implement PA at their level. There are also a few towns that implement PA by themselves. Moreover, Tokyo special districts also implement PA programs. However, I exclude data from prefectures, towns that have their own welfare offices, and the Tokyo special districts from the sample. I also excluded 15 cities that merged between FY1998 and FY2003. In addition, for 59 cities, data on caseworkers are not available for every period of FY1998-2003; these cities are also excluded. As a result, I use the panel of 598 cities during the period of FY1998-2003.

Given this relatively large sample size (598×6), I intended to include as many relevant control variable as possible. However, there are limited data available on city-level variables, with the exception of the years of the national census (which, in the sample, is FY2000). Therefore, the controls are limited in this estimation to unemployment rate, the ratio of elderly people (≥ 65) to the total population, the natural logarithm of population, the natural logarithm of per capita taxable income, a measure of local fiscal climate, and time dummies. These controls are obtained as follows.

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areas of localities in the sample did not change over time.

Unemployment rates at a municipal level are only available in the years of national census. Since unemployment rate is the key variable in the literature, there is no choice but to use the prefectural data, assuming that all cities in a given prefecture will face the same unemployment rate at the level of the prefecture to which they belong. On one hand, since, in Japan, a local labor market usually covers a region than consists of more than one city, prefectural data may well be a good approximation. On the other hand, making this assumption may cause a measurement error. However, as Deaton (1997) shows, this type of measurement error does not lead to inconsistency in parameter estimation. The prefectural data are obtained from the Statistics Bureau (2010).

The data for the other control variables are all at the municipal level. The data regarding the population that are aged 65 or more and for the total municipal population are retrieved from the System of Social and Demographic Statistics (SSDS: *Shyakai Jinko Tokei Takei*), a database maintained by the Statistics Bureau. Per capita taxable income is calculated by taking the ratio of taxable income (the only income variable available at the municipal level) to the number of income tax payers within a municipality. These two variables are also obtained from the SSDS. A popular measure for evaluating municipal fiscal climates in Japan is the fiscal capacity index (FCI), which aims to represent baseline fiscal capability in terms of the three-year average of standardized local tax revenue relative to standardized fiscal demand. The data for FCI are obtained from MIC (various years). The summary statistics of the above variables are listed in Table 1.

Table 1.

3.3. The estimation and econometric issues

As usual with the panel estimation with endogenous regressors, I first eliminate the unobserved heterogeneity by transforming (1a) into a first-difference form as:

$$\Delta y_{it} = \alpha \cdot \Delta y_{it-1} + \beta \cdot \Delta x_{it} + \sum_k \gamma_k \cdot \Delta w_{k.it} + \Delta u_{it}, \qquad (1b)$$

Note that the differenced lagged caseload index ($\Delta y_{it-1} = y_{it-1} - y_{it-2}$) is now correlated with the differenced error ($\Delta u_{it} = u_{it} - u_{it-1}$) in (1b), since u_{it-1} affects y_{it-1} even if u_{it} has no serial correlations. In addition, workload (caseload per caseworker) becomes endogenous if a caseworker changes his/her caseload (the numerator of x_{it}) by implicitly rationing PA benefits. Since this means that x_{it} is then correlated with u_{it}, so then is Δx_{it} with Δu_{it}. In addition, the number of caseworkers (the denominator of x_{it}), which is likely to be fixed at the start of a fiscal year (Nakajima and Arakawa 2004), may also be affected by the caseload of the previous year y_{it-1}, resulting in a differenced workload ($\Delta x_{it} = x_{it} - x_{it-1}$) that is correlated with the differenced error term ($\Delta u_{it} = u_{it} - u_{it-1}$). This is because u_{it-1} affects the denominator of x_{it} via y_{it-1}.

To allow for these two types of endogeneity, I employ the GMM estimator developed by Arellano and Bond (1991), which exploits specific sets of instruments as well as the short length of time-series dimension of a panel data. In the current study, the sample is taken from a six-year period (FY1998 to FY2003). Note also that both the differencing and the inclusion of y_{it-1} as an explanatory variable reduce the sample length to four years, starting in the FY2000. This means that, if u_{it} is not serially correlated, the sets of valid instruments for Δy_{it-1} are obtained as follows: (i) y_{i1998} for Δy_{i1999}; (ii) y_{i1998} and y_{i1999} for Δy_{i2000}; (iii) y_{i1998}, y_{i1999}, and y_{i2000} for Δy_{i2001}; (iv) y_{i1998}, y_{i1999}, y_{i2000}, and y_{i2001} for Δy_{i2002}; and (v) y_{i1998}, y_{i1999}, y_{i2000}, y_{i2001}, and y_{i2002} for Δy_{i2003}. The instruments for workload are all analogously obtained except where one further lag is required since workload is contemporaneously correlated with error term. This gives the following: (i) x_{i1998} for Δx_{i2000}; (ii) x_{i1998} and x_{i1999} for Δx_{i2001}; (iii) x_{i1998}, x_{i1999}, and x_{i2000} for Δx_{i2002}; and (iv) x_{i1998}, x_{i1999}, x_{i2000}, and x_{i2001} for Δx_{i2003}.

In the following estimation, I employ the two-step version of the AB estimator. I also use a robust variance-covariance estimator (VCE), adjusted for clustering on each city. Therefore, the tests for the over-identifying restrictions are not conducted with the robust VCE, since the test statistic in this case does not asymptotically follow the standard chi-squared distribution. The validity of the instruments is thus examined using tests for non-existence of serial correlation in the error term. For this, I use the Arellano-Bond test for the non-existence of the *second* order serial correlation of the *differenced* error term in (1b). If the test does not reject the null hypothesis of no serial correlation, the estimation is regarded as valid.

3.4. The estimation result

For the dependent variable, I use the log of the ratio of caseload to population as well as the log of caseload. Furthermore, I use two subcategories of caseload: PA households with heads who are aged over 65 (aged caseload) and PA households with heads aged 64 or under (non-aged caseload). To make the workload correspond to these two, the workload numerator is replaced with the corresponding caseload. The denominator (the number of caseworkers) is kept the same, as it is impossible to subcategorize caseworkers so that they correspond to these two subcategories.

The results for the calculation of per capita caseload are listed in Table 2a. The three models with the once-lagged dependent variable (1a, 2a, 3a) were found to perform poorly since they reject the non-existence of the second order serial correlation, which indicates the failure of the moment conditions. I then performed analogous sets of estimation that additionally included the twice-lagged dependent variable (1b, 2b, 3b). While the test for the serial correlation still rejects non-existence of the serial correlation for the total caseload (1b), it does not do so when the aged caseload (2b) and the non-aged caseload (3b) are estimated separately. However, workload exerts no significant influence on caseload in these two cases. In addition, some coefficients on the control variables have statistically significant but unexpected signs. The coefficient on the aged-population share is statistically significant but its sign is unexpectedly negative for (2b) and positive for (3b). On the other hand, all controls except aged-population share and twice-lagged dependent variables are not found to be statistically significant for (3b). Given these sets of results in Table 2a, I argue that per capita caseload does not describe the effects on caseload very well.

Table 2a

However, the results change for the non-aged caseload if per capita caseload is replaced with caseload as in Table 2b. Here, the models with the once-lagged dependent variable again perform badly for total caseload (4a) and aged caseload (5a), rejecting the non-existence of the second order serial correlations. However, the test does not reject the null hypothesis for the non-aged caseload (6a). In addition, workload exerts a significant negative impact on non-aged caseload, albeit at the .10 level. For the sake of comparison, I again performed three sets of estimation that additionally include a twice-lagged dependent variable (4b, 5b, 6b). This time, the non-existence of the second order serial correlation is not rejected for all cases. While workload is still insignificant for the total caseload (4b) and the aged caseload (5b), it is significant for the non-aged caseload (6b), albeit at the .10 level again.

Table 2b

Among the control variables, the log of population is the only variable that is statistically significant in all six cases; this is to be expected, since, *ceteris paribus*, a larger population implies more caseloads. However, all other controls were found to be insignificant for the non-aged caseload (6a-b), including unemployment rate. This may be a result of the very short panel (from FY2000 to FY2003) that was used. If these controls did not change very much during this period, the unobserved heterogeneity may absorb their effects on caseload. In addition, the insignificance (and unexpected sign) of the unemployment rate may also be due to the fact that prefectural-level data rather municipal-level data were used; this may have resulted in less data variation in the cross-section dimension.

For the estimation results on the significant effect of workload for the non-aged caseload, I choose (6a) between (6a) and (6b), since (6a) does not reject the second order serial correlation in the first place, and the two lagged dependent variables in (6b) are not statistically significant.

I then performed a series of robustness checks on (6a); these are given in Table 3. Although the table does not exhaust all combinations of the control variables, the effects of workload were found to be fairly robust to different sets of the controls, with statistically significant coefficients ranging from −.120 to −.113. In other words, the result implies that a one percent increase in workload (caseloads per caseworker) reduces non-aged caseloads by just over than 0.1 percent. Some impression of the quantitative volume of the rationing may be obtained by using the sample means in Table 1. For example, the estimation implies that a 10-percent increase ($6.7 = 67 \times .1$) in workload at the sample average of 67 cases would exclude about 5 non-aged cases ($5.22 = 522 \times .001 \times 10$) from the average sample caseload of 522. It is open to interpretation as to whether this result is large or not. However, I argue that the result constitutes evidence for the existence of assistance rationing due to understaffed welfare agencies. Furthermore, this result runs counter to the findings by Suzuki and Zhou (2007), which argued that larger workloads result in larger caseloads, a claim consistent with that has also been made by the MHLW.

On the other hand, workload is shown not to affect *aged*-caseload. This result is plausible. Old households receive PA benefits when their old-age pension benefits are lower than what that which the PA system considers the minimum costs of living. In particular, the PA eligibility assessment for those who are aged 65 or more is almost automatic, since they are not considered to be able to work and it is not difficult to check their old-pension payments. In other words, the means-tests for elderly people are not difficult to perform, and caseworkers may assume that the elderly should be treated differently from non-aged recipients that are potentially able to return to the labor market. Therefore, aged-caseload could be thought of as independent of workload.

Table 3

4. The effects of caseworkers on caseloads

Next, I examine the effects of caseload size, y, on the number of caseworkers, z, which are modeled as

$$z_{it} = \rho \cdot z_{it-1} + \theta \cdot y_{it-1} + \sum_k \varphi_k \cdot w_{k.it} + d_i + u_{it}, \tag{2a}$$

where the symbols are defined as in the previous section. Note that caseload is lagged; this is done so to examine the feedback from caseload in the previous period on the number of caseworkers in the current period.

The model (2a) is again differenced to eliminate the unobserved heterogeneity, as follows:

$$\Delta z_{it} = \rho \cdot \Delta z_{it-1} + \theta \cdot \Delta y_{it} + \sum_k \varphi_k \cdot \Delta w_{k.it} + \Delta u_{it}. \tag{2b}$$

In this case, Δy_{it-1} are endogenous, since, as discussed in the previous section, there are feedbacks from the past number of caseworkers to the current caseloads. The lagged dependent variable Δz_{it-1} is also endogenous, as has been explained previously. Relevant instruments for Δz_{it-1} and Δy_{it-1} for the Arellano-Bond estimation are constructed analogously to those for Δy_{it-1} and Δx_{it-1} that were described in the previous section.

The results are listed in Table 4, where caseload is defined as an explanatory variable, which is categorized again into total, aged, and non-aged caseloads. Given the P values for the test for the non-existence of the second order serial correlation of the differenced error terms in (2b), the moment conditions for the AB estimation are considered to be satisfied for the models with the once-lagged dependent variable.

Table 4

Table 4 shows that the effect of the lagged number of caseworkers on *total* caseload is significant and positive. However, the analogous effects of aged and non-aged caseloads are not statistically significant. This suggests that the number of caseworkers does respond to changes in total caseload size, but does not respond to the volumes of its subcategories (i.e., aged and non-aged). However, none of the control variables, including year effects, had statistically significant effects in all the three types of caseload, except per capita income when non-aged caseload is a regressor (the far right column). These insignificant control variables may, again, be due to the fact that the estimation relies on short a panel, which extends only from FY2000 to FY2003. If these controls do not change very much during this period, the unobserved heterogeneity is likely to absorb the effects on caseload. On the other hand, per capita income has a significantly positive impact when non-aged caseload is a regressor. This may be because the income variable reflects the resources that are available to localities as its value depends on *taxable* income: more fiscally abundant localities (after controlling the fiscal capacity index) are likely to be able to employ more caseworkers. Lastly, the lagged number of caseworkers is statistically significant in all of the three cases with coefficients from .705−.870, which implies rather sluggish adjustments of the number of caseworkers.

For total caseload, I again performed a series of robustness checks (see Table 5). Although the table does not give an exhaustive view of all the combinations of the control variables, the effect of the number of caseworkers was found to be fairly robust in many different sets of the control variables, with statistically significant coefficients ranging from .323 to .398. It may also be worth noting that none of the control variables, except year effects, becomes significant even when the combinations change. In addition, the fact that year effects become significant in some combinations of controls may substantiate the argument that the unobserved heterogeneity would absorb the effects of the control variables, and that the insignificance of the time effects in Table 4 may reflect multicollinearity between the year effects and the other controls.

Table 5

In any event, Table 5 indicates that the effect of caseload size on the number of caseworkers is fairly robust. Results in Tables 4 and 5 imply that a one percent increase in caseload in the previous period will increase the number of caseworkers in the current period by about .3 or .4 percent. This is indeed evidence that localities respond to an increase in caseload by increasing the number of caseworkers they employ. However, this response may be considered to be very limited. Using the mean values in Table 1, a 10-percent increase from the average value of 956 caseloads (a 95.6-case increase) would increase the number of caseworkers *by a maximum* of less than one ($0.48 = 12 \times 0.004 \times 10$), from the sample average of 12 caseworkers. These results imply that, even if a caseload increases by almost 100, an average locality may well not even employ one additional caseworker.

5. Concluding remarks

While a number of empirical studies in the US have explored the determinants of welfare caseloads, none of the studies has examined the effect of workload on caseload. However, several studies from outside the field of economics have suggested that workload can play an important role in caseload size, implying that higher workloads may well lead to the *rationing* of assistance; that is, more caseloads per caseworker *decreases* the total number of caseloads.

This paper has examined the effect of workload on caseload size to examine the existence of the rationing of social assistance benefits, and shown evidence for the rationing of assistance. The estimate has indicated that a one-percent increase in workload (caseloads per caseworker) reduces non-aged caseloads by just over .1 percent. For example, when evaluated at the sample mean, this implies that a 6.7-workload increase would result in a reduction of more than five non-aged assistance applications. In particular, it is worth drawing attention to the fact that this result runs counter to the findings of the previous Japanese study in this area, which argued that greater workload leads to a higher caseload, which has also been claimed by the MHLW.

I have also examined the effects of caseload size on the number of caseworkers. The results imply that one percent increase in caseload in the previous period increase caseworkers in the current period by about .3 − .4 percent, showing that localities respond to an increase in caseload by increasing their caseworkers. However, this response rate may be considered to be very small. Evaluated at the sample mean, the estimate implies that, even if caseload increased by almost 100, localities might even not add even one caseworker. This also implies that localities are quite sluggish in adjusting the number of caseworkers to most effectively deal with an increase in caseload; this suggests that the policy claim by the MHLW is *partly* supported.

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Figure 1. Distribution of workload (caseload/caseworker) FY2007

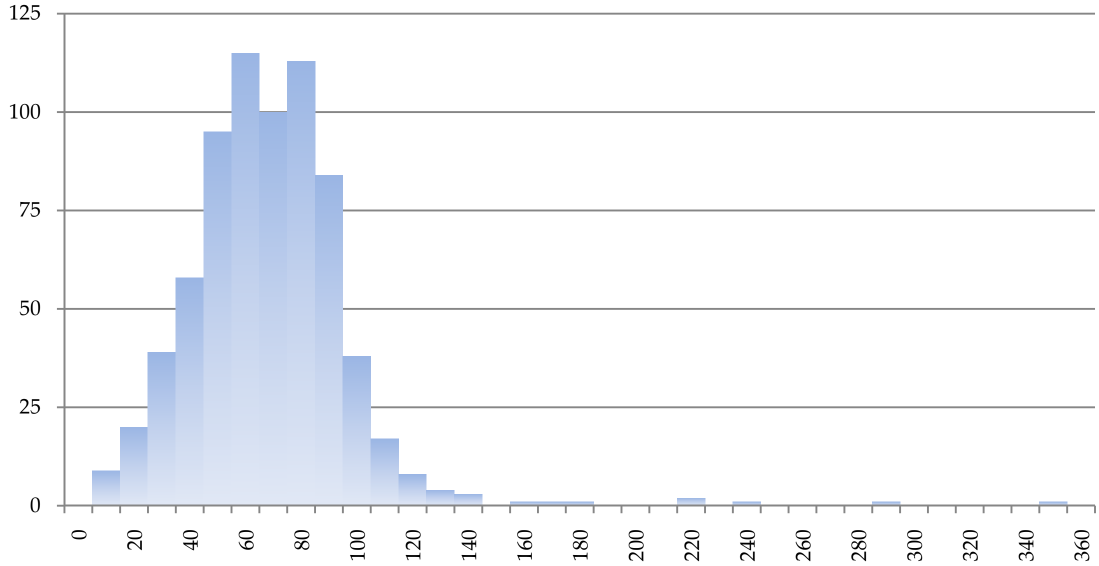

Table 1. Sample statistics: FY1998–FY2003

Variable		mean	std. dev.	min.	max.	obs.
#caseloads (household)	overall	956	3,235	18	70,210	$N \times T$ = 3,588
	between		3,198	21	54,446	N = 598
	within		507	−11,550	16,720	T = 6
#caseloads with heads aged ≥ 65. (household)	overall	433	1,553	7.0	36,357	$N \times T$ = 3,588
	between		1,530	10.3	27,909	N = 598
	within		272	−7,105	8,881	T = 6
#caseloads with heads aged < 65. (household)	overall	521.7	1,701.7	6.0	33,805.0	$N \times T$ = 3,588
	between		1,685.7	7.8	26,493.8	N = 598
	within		241.2	−4,440.2	7,832.8	T = 6
#caseworkers (person)	overall	11.61	32.55	1.00	445.00	$N \times T$ = 3,588
	between		32.41	1.00	414.17	N = 598
	within		3.15	−55.55	74.45	T = 6
#caseload/#caseworkers	overall	67.15	32.01	7.20	637.00	$N \times T$ = 3,588
	between		29.77	10.25	448.67	N = 598
	within		11.81	−68.52	255.48	T = 6
#caseload with heads aged ≥ 65 / #caseworkers	overall	30.38	14.06	3.00	209.00	$N \times T$ = 3,588
	between		12.90	5.50	153.17	N = 598
	within		5.61	-15.79	105.52	T = 6
#caseload with heads aged < 65 / #caseworkers	overall	3.48	0.52	1.10	6.06	$N \times T$ = 3,588
	between		0.49	1.35	5.65	N = 598
	within		0.17	2.65	4.41	T = 6
Unemployment rate	overall	0.05	0.01	0.02	0.08	$N \times T$ = 3,588
	between		0.01	0.03	0.08	N = 598
	within		0.01	0.03	0.06	T = 6
Proportion of those aged ≥ 65 among population	overall	0.186	0.048	0.066	0.455	$N \times T$ = 3,588
	between		0.047	0.079	0.335	N = 598
	within		0.011	0.139	0.370	T = 6
Population (person)	overall	139,658	256,337	5,799	3,466,875	$N \times T$ = 3,588
	between		256,497	6,118	3,392,694	N = 598
	within		3,087	72,180	213,839	T = 6
Per capita taxable income (000 yen)	overall	1,365.3	296.2	659.5	2,767.4	$N \times T$ = 3,588
	between		290.4	692.1	2,629.7	N = 598
	within		59.3	1,188.7	1,539.9	T = 6
Fiscal capacity index	overall	0.6694	0.232	0.1	1.6	$N \times T$ = 3,588
	between		0.231	0.1	1.53	N = 598
	within		0.026	0.509	0.988	T = 6

Table 2a Estimation results: dependent variable = log(caseload/population)

Dependent variable / Explanatory variable	Log(caseload/population)					
	(1) Total		(2) Aged ≥ 65		(3) Non-aged	
	a	b	a	b	a	b
Log(workload)	−.018 (.032)	−.031 (.030)	−.010 (.041)	.062 (.047)	−.063 (.056)	.002 (.072)
Unemployment rate	−.599 (.382)	−.432 (.570)	−.770 (.628)	−.838 (.722)	−.531 (.644)	.530 (.941)
Share aged (≥65) population	−.744**** (.232)	−.313 (.402)	−4.408**** (.511)	−3.582**** (.347)	.825 (.504)	.935* (.521)
Log(population)	.410 (.305)	.078 (.490)	.086 (.236)	.552*** (.241)	.336 (.402)	−.111 (.471)
Log(per capita income)	−.192* (.100)	−.118 (.123)	−.291** (.140)	−.449**** (.154)	−.084 (.151)	.081 (.171)
Fiscal capacity index	.070 (.071)	.142** (.072)	.041 (.105)	−.129 (.106)	.001 (.112)	.176 (.118)
Log(caseload/ population)[t–1]	.635**** (.058)	1.317**** (.207)	.792**** (.069)	.999**** (.100)	.570**** (.106)	1.054**** (.234)
Log(caseload/ population)[t–2]		−.408**** (.110)		−.142**** (.044)		−.206**** (.072)
Year effect (2000)	.030**** (.004)		.020**** (.007)		.028**** (.008)	
Year effect (2001)	.056**** (.007)	.010 (.007)	.077**** (.011)	.050**** (.005)	.049**** (.014)	−.001 (.014)
Year effect (2002)	.095**** (.011)	.018* (.015)	.099**** (.017)	.054**** (.013)	.087**** (.022)	.019 (.027)
Year effect (2003)	.118**** (.016)	.028 (.024)	.122**** (.024)	.064**** (.020)	.117**** (.030)	.028 (.041)
H0: No 2nd order serial correlation (P-value)	.001	.017	.032	.129	.032	.916
# instruments	29	26	29	26	29	26
# observations	2,392	1,794	2,392	1,794	2,392	2,392
# groups (N)	598	598	598	598	598	598
# periods (T)	4	3	4	3	4	3

Notes: Estimates are the two-step Arellano-Bond GMM estimates. Robust standard errors are in parentheses, adjusted for clustering on each group. Asterisks ****, ***, **, and * indicate statistical significance at the .01, .025, .05, and .10 levels, respectively.

Table 2b Estimation results: dependent variable = log(caseload)

Dependent variable / Explanatory variable	Log(caseload)					
	(4) Total		(5) Aged ≥ 65		(6) Non-aged	
	a	b	a	b	a	b
Log(workload)	−.019	−.028	−.031	.068	−.114*	−.169*
	(.032)	(.031)	(.045)	(.057)	(.068)	(.092)
Unemployment rate	−.680**	−.495	−.908	−.693	−.481	−.163
	(.342)	(.447)	(.595)	(.705)	(.629)	(.966)
Share aged (≥65) population	−.007	−.041	−.098****	−.294	.013	.069
	(.237)	(.296)	(.430)	(.594)	(.413)	(.159)
Log(population)	.769****	.828****	.542**	.455*	1.145****	1.388****
	(.155)	(.293)	(.236)	(.262)	(.219)	(.438)
Log(per capita income)	−.074	−.052	−.312**	−.343***	−.080	.058
	(.085)	(.108)	(.145)	(.150)	(.132)	(.159)
Fiscal capacity index	−.054	.009	.018	−.070	.179	−.135
	(.070)	(.093)	(.105)	(.112)	(.115)	(.167)
Log(caseload)[*t*−1]	.525****	.568*	.730****	1.161****	.262***	−.002
	(.050)	(.207)	(.080)	(.153)	(.106)	(.386)
Log(caseload)[*t*−2]		−.024		−.211****		.081
		(.182)		(.058)		(.114)
Year effect (2000)	.030****		.016*		.040****	
	(.004)		(.007)		(.008)	
Year effect (2001)	.065****	.033****	.042****	.005	.082****	.056****
	(.007)	(.011)	(.015)	(.010)	(.015)	(.019)
Year effect (2002)	.110****	.077****	.066****	.003	.142****	.130****
	(.011)	(.024)	(.023)	(.021)	(.024)	(.039)
Year effect (2003)	.144****	.109****	.075***	−.014	.198****	.206***
	(.016)	(.041)	(.031)	(.031)	(.033)	(.061)
H0: No 2nd order serial correlation (*P*-value)	.015	.418	.002	.106	.985	.402
# instruments	29	26	29	26	29	26
# observations	2,392	1,794	2,392	1,794	2,392	2,392
# groups (*N*)	598	598	598	598	598	598
# periods (*T*)	4	3	4	3	4	3

Notes: Estimates are the two-step Arellano-Bond GMM estimates. Robust standard errors are in parentheses, adjusted for clustering on each group. Asterisks ****, ***, **, and * indicate statistical significance at the .01, .025, .05, and .10 levels, respectively.

Table 3 Robustness-check: dependent variable = log(non-aged caseload)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Log(workload)	−.122*	−.126*	−.129*	−.125*	−.126*	−.118*	−.124*	−.129*	−.121*	−.123*	−.113*	−.121*
	(.071)	(.072)	(.073)	(.070)	(.071)	(.071)	(.070)	(.073)	(.072)	(.070)	(.068)	(.072)
Unemployment rate		−.861	−.846	−.495	−.877	−.846	−.490	−.862	−.829	−.512	−.461	−.841
		(.643)	(.644)	(.641)	(.642)	(.634)	(.640)	(.643)	(.633)	(.640)	(.628)	(.632)
Share aged (≥65) population			−.349				.067	−.337	−.399	.073	.005	.388
			(.560)				(.432)	(.556)	(.569)	(.433)	(.413)	(.565)
Log(population)				1.135****			1.148****			1.151****	1.142****	
				(.227)			(.222)			(.222)	(.219)	
Log(per capita income)					−.067			−.061		−.100		−.039
					(.133)			(.134)		(.132)		(.132)
Fiscal capacity index						−.195*			−.203*		−.190*	−.198*
						(.118)			(.118)		(.113)	(.118)
Log(caseload) [t−1]	.323****	.317****	.316****	.262****	.319****	.309****	.262****	.318****	.309****	.263****	.261****	.309****
	(.105)	(.106)	(.106)	(.106)	(.105)	(.106)	(.106)	(.106)	(.106)	(.105)	(.107)	(.106)
Year effect (2000)	.043****	.046****	.048****	.046****	.043****	.041****	.045****	.046****	.044****	.043****	.042****	.043****
	(.007)	(.007)	(.009)	(.007)	(.008)	(.007)	(.008)	(.010)	(.009)	(.008)	(.008)	(.010)
Year effect (2001)	.084****	.089****	.093****	.091****	.086****	.083****	.090****	.090****	.087****	.086****	.085****	.086****
	(.014)	(.015)	(.018)	(.015)	(.016)	(.015)	(.015)	(.018)	(.017)	(.016)	(.015)	(.018)
Year effect (2002)	.141****	.150****	.155****	.153****	.145****	.145****	.152****	.151****	.152****	.146****	.147****	.149****
	(.022)	(.024)	(.027)	(.024)	(.024)	(.023)	(.024)	(.027)	(.027)	(.024)	(.023)	(.027)
Year effect (2003)	.195****	.203****	.211****	.211****	.196****	.201****	.209****	.205****	.211****	.198****	.207****	.207****
	(.030)	(.032)	(.036)	(.031)	(.033)	(.031)	(.032)	(.038)	(.037)	(.033)	(.032)	(.038)
H_0: No 2nd order serial correlation (*P*-value)	.810	.779	.789	.937	.741	.794	.942	753	.810	.992	.984	.787

Notes: The estimation methods for the coefficients and the standard errors are those for Table 2. Asterisks ****, ***, **, and * indicate statistical significance at the .01, .025, .05, and .10 levels, respectively.

Table 4 Estimation results: log(caseworker)

Explanatory variables \ Dependent variable	Log(caseworker)		
Log(caseload: Total)[t–1]	.357**** (.115)		
Log(caseload: Aged ≥ 65)[t–1]		.252 (.211)	
Log(caseload: Non-aged)[t–1]			.234 (.200)
Unemployment rate	.515 (1.334)	1.391 (1.433)	.611 (1.403)
Share aged (≥65) population	1.250 (2.044)	1.820 (2.018)	1.818 (2.254)
Log(population)	−.613 (.524)	−.720 (.636)	−.299 (.548)
Log(per capita income)	.363 (.253)	.372 (.266)	.520** (.261)
Fiscal capacity index	−.392 (.285)	−.439 (.289)	−.261 (.297)
Log(caseworker)[t–1]	.705**** (.089)	.870**** (.128)	.749**** (.101)
Year effect (2000)	−.026 (.019)	−.029 (.025)	−.014 (.023)
Year effect (2001)	−.031 (.027)	−.038 (.041)	−.008 (.033)
Year effect (2002)	−.044 (.041)	−.055 (.066)	−.011 (.050)
Year effect (2003)	−.041 (.058)	−.051 (.090)	.008 (.072)
H_0: No 2nd order serial corr. (P value)	.205	.152	.162
#instruments	29	29	29
# observations	2,392	2,392	2,392
# groups (N)	598	598	598
# periods (T)	4	4	4

Notes: Estimates are the two-step Arellano-Bond GMM estimates. Robust standard errors are in parentheses, adjusted for clustering on each group. Asterisks ****, ***, **, and * indicate statistical significance at the .01, .025, .05, and .10 levels, respectively.

Table 5 Robustness-check: dependent variable = log(caseworker)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Log(total caseload)[t−1]	.355****	.355****	.357****	.398****	.347****	.325****	.390****	.349****	.330****	.323****	.360****	.388****
	(.108)	(.106)	(.106)	(.115)	(.107)	(.111)	(.112)	(.107)	(.111)	(.112)	(.115)	(.111)
Unemployment rate		.743	.668	.842	.752	.603	.704	.680	.564	.585	.521	.694
		(1.312)	(1.307)	(1.350)	(1.316)	(1.310)	(1.336)	(1.309)	(1.303)	(1.304)	(1.333)	(1.338)
Share aged (≥65) population			1.596				1.399	1.549	1.514	1.416	1.364	1.338
			(2.204)				(2.369)	(2.164)	(2.125)	(2.090)	(2.132)	(2.282)
Log(population)				−.640			−.510				−.552	−.585
				(.652)			(.574)				(.529)	(.569)
Log(per capita income)					.312			.296		.362		.332
					(.247)			(.245)		(.249)		(.252)
Fiscal capacity index						−.353			−.335	−.347	−.365	
						(.281)			(.265)	(.266)	(.282)	
Log(caseworker) [t−1]	.720****	.725****	.715****	.725****	.728****	.705****	.715****	.719****	.696****	.697****	.702****	.722****
	(.087)	(.086)	(.091)	(.089)	(.086)	(.081)	(.096)	(.091)	(.085)	(.085)	(.089)	(.096)
Year effect (2000)	−.017*	−.018*	−.030	−.020**	−.010	−.023***	−.030	−.022	−.034*	−.026	−.035*	−.022
	(.009)	(.009)	(.020)	(.010)	(.011)	(.009)	(.020)	(.020)	(.020)	(.020)	(.019)	(.021)
Year effect (2001)	−.018	−.021	−.038	−.027*	−.009	−.026**	−.040	−.026	−.043	−.029	−.045*	−.027
	(.013)	(.014)	(.027)	(.015)	(.018)	(.013)	(.027)	(.028)	(.026)	(.028)	(.026)	(.028)
Year effect (2002)	−.032	−.039*	−.065	−.048**	−.019	−.039*	−.068	−.045	−.064	−.042	−.067*	−.047
	(.020)	(.022)	(.041)	(.022)	(.028)	(.022)	(.041)	(.043)	(.040)	(.043)	(.039)	(.043)
Year effect (2003)	−.041	−.047*	−.083	−.060**	−.014	−.041	−.088	−.051	−.075	−.040	−.080	−.053
	(.062)	(.027)	(.056)	(.029)	(.039)	(.029)	(.056)	(.061)	(.055)	(.061)	(.053)	(.061)
H0: No 2nd order serial correlation (P-value)	.165	.168	.191	.171	.167	.185	.191	.190	.209	.209	.205	.188

Notes: The estimation methods for the coefficients and the standard errors are those for Table 2. Asterisks ****, ***, **, and * indicate statistical significance at the .01, .025, .05, and .10 levels, respectively.