This item is the archived peer-reviewed author-version of:

What does variation in survey design reveal about the nature of measurement errors in household consumption?

Reference:

Gibson John, Beegle Kathleen, De Weerd Joachim, Friedman Jed.- What does variation in survey design reveal about the nature of measurement errors in household consumption?

Oxford bulletin of economics and statistics - ISSN 0305-9049 - 77:3(2015), p. 466-474

Full text (Publisher's DOI): <http://dx.doi.org/doi:10.1111/OBES.12066>

To cite this reference: <http://hdl.handle.net/10067/1263430151162165141>

November, 2013

What Does Variation in Survey Design Reveal About the Nature of Measurement Errors in Household Consumption?

John Gibson, University of Waikato

Kathleen Beegle, World Bank

Joachim De Weerd, EDI Tanzania

Jed Friedman, World Bank

Abstract

We randomly assigned eight different consumption surveys to obtain evidence on the nature of measurement errors in estimates of household consumption. Regressions using data from more error-prone designs are compared with results from a “gold standard” survey. Measurement errors appear to have a mean-reverting negative correlation with true consumption, especially for food and especially for rural households.

JEL: C21, C81, D12

Keywords: Consumption, Engel curves, Household surveys, Measurement error

Corresponding Author: John Gibson, Department of Economics, University of Waikato.
Ph: 64-7-838-4289. Email:

I. Introduction

Household consumption surveys are crucial for measuring living standards, especially in poor countries where labor and income surveys cover neither the full population nor all activity. The complexity of consumption surveys makes measurement error likely but validation studies to reveal the nature of errors are mainly for labor market settings, using either administrative data (Bound and Krueger, 1991) or firm records (Pischke, 1995). Also, such studies are limited to rich countries, including for the one type of consumption – health expenditure – for which validation has been attempted (Cohen and Carlson, 1994).

This note reports on a survey experiment in Tanzania that may inform practitioners about the nature of measurement error in developing country consumption data. Eight different survey designs were randomly assigned to 4032 households (three households per design in 168 sampling units), covering urban and rural areas. One resource intensive design – individually kept diaries with daily supervision for 14 days – approximates a “gold standard” and is used by Beegle et al (2012) to examine the effects of different survey designs on poverty and inequality statistics (but without an explicit focus on the nature of the measurement errors). The tracking of all commodity in-flows (harvests, purchases, gifts, destocking), out-flows (sales, gifts, restocking, food fed to animals), daily attendance at meals, and acquisitions and disposals by dependents the diary-keeper reported on should minimize recall bias. Survey diagnostics, such as the time profile for diary entries and daily consumption, show no diary fatigue for this “gold standard” module and just three households that started a diary did not complete the survey.¹ Therefore we believe that it is reasonable to assume that data from the “gold standard” method is as close to error-free as is practicable for field surveys in developing countries.

¹Of 4032 households assigned questionnaires, just 13 were replaced due to refusal. The final sample was 4025.

Beyond random assignment, several steps were taken to ensure that differences in measured consumption are solely due to survey design. Small, closely-supervised teams of experienced interviewers conducted the experiment over 12 months to balance each module over time and space. Each team implemented all eight modules in equal proportion so as to not confound module effects with interviewer effects. To prevent potentially uncontrollable cross-module spillovers within households, each household faced just a single survey design.²

The “gold standard” measure is not available for each household so we use two indirect methods to study the nature of measurement errors.³ First, we average the logarithm of total household consumption, for each sampling unit (hereafter, ‘village’) and each survey design. If measurement errors in consumption are random, averaging should reduce bias, with estimates converging to true village averages. We find that errors are not random. Subtracting the average for the “gold standard” from averages of the other seven modules shows that differences are negatively correlated with the benchmark (true) village average. Exactly the same pattern, found with earnings data, is referred to by Bound and Krueger (1991) as *mean-reverting measurement error*.

Practitioners rarely work with village averages so in our second exercise we estimate a widely used regression on household consumption data – a food Engel curve. The Engel curve is useful here because of extant results on expected biases from different types of measurement error, motivated by a puzzle raised by Deaton and Paxson (1998).⁴ The

²Previous comparisons of consumption survey designs apply them sequentially to the same household (e.g., Ahmed et al, 2010). Sequential comparisons lack balance in timing and may have *conditioning bias*, where those previously recording in diaries may be atypically accurate recall respondents, and those initially surveyed by recall might shirk from daily recording in diaries.

³Our methods would be considered ‘macro-level comparisons’ of survey estimates versus estimates generated under preferred survey conditions in the Bound et al (2001) typology of validation studies. The problems such comparisons face do not apply to our experiment given the balance we achieved over time, space, and samples.

⁴The puzzle is that food shares fall as household size rises at constant per capita consumption. The effective income increase from sharing public goods in larger households should outweigh the substitution effect (public goods are cheaper in large households) so food shares should rise for the poor. But Deaton and Paxson find the

estimates from the “gold standard” are treated as closest to what true consumption would reveal and are compared with estimates from the other designs, and from data with simulated random and non-random measurement errors. This exercise also shows that the most plausible pattern is mean-reverting errors negatively correlated with true consumption.

Mean-reverting errors bias regression coefficients even if they are just present in the dependent variable. Consumption is often an outcome measure in impact evaluations, so understated impacts may result. Consumption is also a key explanatory variable in many studies (to proxy permanent income), and here mean-reverting errors will bias regression coefficients either toward or away from zero. In contrast, classical errors cause no bias when just in the dependent variable, and attenuate the coefficient of a single error-ridden explanatory variable. Also, practitioners’ main correction for measurement error bias – instrumental variables (IV) – is inconsistent for mean-reverting errors (Black, Berger and Scott, 2000), and bounding estimates based on reverse regression are unlikely to be effective in practice (Gibson and Kim, 2010).

II. Motivation: Effects of Random and Non-random Measurement Errors

Consider some true model: $y = \alpha + \beta x + u$, for an outcome y , an independent variable x , (which may indicate treatment), a response coefficient β , and a pure random error, u . The observed value of the outcome variable y^* is related to the true value by:

$$y^* = \theta + \lambda y + v. \quad (1)$$

The textbook case of classical measurement error places stringent restrictions on equation (1), specifically that $\theta = 0$, $\lambda = 1$ and $E(v) = \text{cov}(y, v) = \text{cov}(x, v) = \text{cov}(u, v) = 0$, so that just

most negative effects of household size in their Engel curves for poor countries. Nothing in the current note resolves the puzzle, since there is a significant negative effect of household size with all survey designs. But the prior literature on the effect of measurement error on the food Engel curve (Gibson, 2002; Gibson and Kim, 2007; Ahmed et al, 2010) provides diagnostics for how different types of errors affect the estimated coefficients.

white noise is added to the true value. In contrast, validation studies of labor survey data find that $0 < \lambda < 1$, which Bound and Krueger (1991) call mean-reverting measurement error.

The estimator of the response coefficient with the error-ridden dependent variable is:

$$\beta_{y^*x} = \frac{\text{cov}(y^*, x)}{\text{var}(x)} = \frac{\text{cov}(\lambda\alpha + \lambda\beta x + \lambda u - v, x)}{\text{var}(x)} = \lambda\beta \quad (2)$$

Thus, if $0 < \lambda < 1$, the estimated response coefficient is attenuated. Many impact evaluations use household consumption as an outcome measure (e.g. Khandker, 2005). Equation (2) suggests that impacts may be understated due to mean-reverting errors.

Consider next the case of no error in the dependent variable (or just white noise error) while the observed value for the independent variable, x^* , is related to the true value by:

$$x^* = \theta + \lambda x + v. \quad (3)$$

The estimator of the response coefficient is then:

$$\beta_{yx^*} = \frac{\text{cov}(y, x^*)}{\text{var}(x^*)} = \frac{\text{cov}(\alpha + \frac{\beta}{\lambda}x^* - \frac{\beta\theta}{\lambda} - \frac{\beta}{\lambda}v + u, x^*)}{\text{var}(x^*)} = \beta \frac{\lambda\sigma_x^2}{\lambda^2\sigma_x^2 + \sigma_v^2} \quad (4)$$

With classical error ($\lambda = 1$) the rescaling of the response coefficient is the familiar attenuation in proportion to the explanatory variable's 'reliability ratio' ($\sigma_x^2 / [\sigma_x^2 + \sigma_v^2]$). But with mean-reverting error, attenuation is not guaranteed. If the 'shrinkage' of the variance in the first term in the denominator due to multiplying by λ^2 (for $0 < \lambda < 1$) exceeds the effect of adding the variance of the random noise term (σ_v^2), the response coefficient is overstated rather than understated (and variance of the error-ridden variable is less than that of the true variable).

Knowing if the coefficient on the proxy for permanent income is either attenuated or exaggerated seems important for many policy conclusions researchers may draw from regressions where surveyed household consumption is a key explanatory variable. Moreover,

when practitioners attempt to deal with possible attenuation by using instrumental variables for household consumption (e.g., Alderman et al, 2006) they are using an estimator that is inconsistent when measurement errors are correlated with true values (Black et al, 2000). Once again, knowing more about the nature of measurement error in household consumption data may help improve modeling practice and interpretation of empirical results.

III. The Survey Experiment and Evidence from Village Averages

The eight designs differ by method of data capture (diary versus recall), by respondent (individual versus household reporting), by recall period (7 day, 14 day and usual month), by number of items in the recall list, and by the universe of consumption that they attempt to cover (Table 1). The designs reflect the most typical consumption modules in multi-topic living standards surveys in developing countries, with design variation restricted to foods and frequently purchased non-foods.⁵ Random assignment balanced over consumption-related characteristics and all modules were fielded within villages at the same time, so no controls are used when comparing village averages. Reported consumption is highest for the “gold standard” design of an intensively supervised, individually-kept diary (Table 2, column 1). The variances reported in column 3 of Table 2 are inconsistent with the assumptions of classical measurement error. For three of the designs (the subset 7-day recall after scaling, and both household diaries) the variance of the error-ridden variable is less than that of the benchmark, which could not happen with random measurement error.

The nature of the error is shown by estimating equation (1) seven times, with village-averaged log total consumption from modules 1 to 7 as the dependent variable, y^* and the village average of log consumption from the benchmark individual diary treated as the true y .

⁵Assignment to different modules also affects reporting on infrequently purchased non-food, which Beegle et al (2012) ascribe to either respondent conditioning or fatigue, since questions on infrequent items came after the lengthy food and frequent non-food recall sections in modules 1-5 and after the two-week diary for modules 6-8.

We find $\hat{\theta} > 0$, $\hat{\lambda} < 1$ in all seven regressions, with $\hat{\lambda}$ ranging between 0.42 and 0.66 and always less than one (columns 5 and 6). The three survey designs with lower variance than in the benchmark design have the most mean-reverting error, with $0.42 < \hat{\lambda} < 0.54$. For these designs, the shrinkage due to the λ^2 term in the denominator of equation (4) outweighs the effect of adding the variance of the random noise term. Furthermore, finding $\hat{\lambda} < 1$ in all regressions implies a negative correlation between errors and true values.

The extent of mean-reversion is strongest in rural areas, with $\hat{\lambda}_{Rural}$ ranging from 0.121 to 0.231 (column 7), and for each module $\hat{\lambda}_{Rural}$ is statistically significantly less than $\hat{\lambda}_{Urban}$ (column 9). Yet it is in rural areas where practitioners are most reliant on household consumption data as a monetary welfare indicator, since alternatives like earnings are available for very few households. Finally, the extent of mean-reversion illustrated in Table 2 does not appear to be an artifact of functional form choices; in column (10) we report results from polynomial specifications (generally cubics) and the elasticity from these polynomials that corresponds to $\hat{\lambda}$ is smaller for five out of the seven survey designs (averaging 0.53 compared with 0.55 for the linear specification results in column (5)).

IV. Evidence from Food Engel Curve Regressions

Our second exercise uses a simplified version of the food Engel curve of Deaton and Paxson (1998), with demographic composition and control variables ignored to reduce clutter:

$$w_{f,i} = \alpha + \beta \ln\left(\frac{x}{n}\right)_i + \gamma \ln n_i + u_i. \quad (5)$$

The food share for household i , $w_{f,i}$ depends on total consumption, x_i household size, n_i and a disturbance, u_i . The data on log per capita consumption, $\ln(x/n)_i$ are affected by

measurement error in x_i and with errors in food and non-food the food share is also affected (Beegle et al (2012) rule out equi-proportional errors). The bias in $\hat{\beta}$ and $\hat{\gamma}$ depends on the relative degree of measurement error in food and non-food and cannot be signed *a priori* except for the special case of classical error (Gibson and Kim, 2007).

We estimate equation (5) with the benchmark diary data, and then compare with coefficients estimated on the data from the other designs to see how measurement error affects $\hat{\beta}$ and $\hat{\gamma}$. We then estimate Engel curves on simulated data to see whether errors that are: (i) random, or (ii) negatively correlated with true values, or (iii) negatively correlated with household size match the empirically observed cross-module pattern. These simulations are motivated and described in Gibson et al (2013), who use the results from the “gold standard” diary to provide parameter values for the simulated error-free data, in keeping with the maintained assumption that these diary data are closest to the truth.

There is significant variation in $\hat{\beta}$ over the different consumption modules (Table 3). The coefficient on log per capita consumption in the benchmark module is -0.026, while in six of the other modules, $\hat{\beta}$ is statistically significantly more negative, ranging from -0.083 to -0.059.⁶ The hypothesis of equal $\hat{\beta}$ across all eight modules is strongly rejected ($p=0.002$). In contrast, there is no module effect on $\hat{\gamma}$, which ranges between -0.037 and -0.072, with no statistically significant differences.

What type of measurement errors could cause a significantly more negative $\hat{\beta}$ and no impact on $\hat{\gamma}$? The simulations show that it requires errors in food consumption to be more strongly (negatively) correlated with true values than are the errors in nonfood consumption.

⁶ The regressions also include the share of children aged less than six, children aged six to fifteen, and elders aged over 65, the age, education, gender and marital status of the household head, and month and district fixed effects. Full results are reported in Gibson et al (2013).

This combination causes the $\hat{\beta}$ estimated from the error-ridden module to be sufficiently more negative than when estimated on error-free data, while leaving $\hat{\gamma}$ largely unchanged. The simulations also rule out the other hypotheses of either random errors or errors negatively correlated with household size. Larger random errors in measuring food consumption make $\hat{\beta}$ less negative rather than more negative. No simulated correlation with household size is strong enough to give negative enough $\hat{\beta}$ values to match those from the Table 3 regressions, even with simulated error-free non-food consumption.

V. Summary and Implications

The nature of measurement errors in household consumption data is largely unknown because of difficulty in validating survey estimates. In this note we provide indirect evidence, using two different regression approaches and an experiment where eight different consumption questionnaires were randomly assigned to households. The results are consistent with errors in measured consumption that are negatively correlated with true values, especially for food and especially in rural areas. Such correlations are plausible since survey reporting tasks become harder for richer household with more varied consumption (Pradhan, 2009). This negative correlation creates mean reversion, so even mis-measured consumption as a dependent variable causes attenuated regression coefficients, and when consumption is an explanatory variable the usual attenuation bias may not apply. Both cases should concern practitioners who rely on accurate household consumption data for their regressions.

References

- Ahmed, A., Brzozowski, M. and Crossley, T. (2010). 'Measurement errors in recall food consumption data.' *Mimeo* University of Cambridge.
- Alderman, H., Hoogeveen, H. and Rossi, M. (2006) 'Reducing child malnutrition in Tanzania: Combined effects of income growth and program interventions.' *Economics and Human Biology* 4(1): 1-23.
- Beegle, K., de Weerd, J., Friedman, J., and Gibson, J. (2012). 'Methods of household consumption measurement through surveys: Experimental results from Tanzania.' *Journal of Development Economics* 98(1): 3-18.
- Black, D., Berger, M., and Scott, F. (2000). 'Bounding parameter estimates with nonclassical measurement error.' *Journal of the American Statistical Association* 95(451): 739-748.
- Bound, J., and Krueger, A. (1991). 'The extent of measurement error in longitudinal earnings data: Do two wrongs make a right?' *Journal of Labor Economics* 9(1): 1-24.
- Bound, J., Brown, C., and Mathiowetz, N. (2001). 'Measurement error in survey data.' In J. Heckman and E. Leamer (eds.), *Handbook of Econometrics: Volume 5*, Elsevier, pp. 3705-3843.
- Cohen, S., and Carlson, B. (1994). 'A comparison of household and medical provider reported expenditures in the 1987 NMES.' *Journal of Official Statistics* 10(1):3-29.
- Deaton, A., and Paxson, C. (1998). 'Economies of scale, household size, and the demand for food.' *Journal of Political Economy* 106(5): 897-930.
- Gibson, J. (2002). 'Why does the Engel method work? Food demand, economies of size and household survey methods.' *Oxford Bulletin of Economics and Statistics* 64(4): 341-360.
- Gibson, J., Beegle, K., de Weerd, J., and Friedman, J. (2013). 'What does variation in survey design reveal about the nature of measurement errors in household consumption?' *Working Paper No 13/01*, Department of Economics, University of Waikato.
- Gibson, J. and Kim, B. (2007). 'Measurement error in recall surveys and the relationship between household size and food demand.' *American Journal of Agricultural Economics* 89(2): 473-489.
- Gibson, J. and Kim, B. (2010). 'Non-classical measurement error in long-term retrospective surveys.' *Oxford Bulletin of Economics and Statistics* 72(5): 687-695.
- Khandker, S. (2005). 'Microfinance and poverty: evidence using panel data from Bangladesh.' *World Bank Economic Review* 19(2): 263-286.
- Pischke, J-S. (1995). 'Measurement error and earnings dynamics: Some estimates from the PSID Validation Study.' *Journal of Business and Economics Statistics* 13(3): 305-314.
- Pradhan, M. (2009). 'Welfare analysis with a proxy consumption measure: Evidence from a repeated experiment in Indonesia.' *Fiscal Studies* 30(3-4): 391-417.

Table 1. Survey experiment consumption modules

Module	Description	Details	Number of households
1	Long list (58 food items) 14 day	Quantity from purchases, own-production, and gifts/other sources; Tshilling value of consumption from purchases	503
2	Long list (58 food items) 7 day	Quantity from purchases, own-production, and gifts/other sources; Tshilling value of consumption from purchases	504
3	Subset list (17 food items; subset of 58 foods), scaled by 1/0.77 ^a 7 day	Quantity from purchases, own-production, and gifts/other sources; Tshilling value of consumption from purchases	504
4	Collapsed list (11 food items covering universe of food categories) 7 day	Tshilling value of consumption	504
5	Long list (58 food items) Usual 12 month	Consumption from purchases: number of months consumed, quantity per month, Tshilling value per month Consumption from own-production: number of months consumed, quantity per month, Tshilling value per month Consumption from gifts/other sources: total estimated value for last 12 months	504
6	Household diary, frequent visits 14 day diary		502
7	Household diary, infrequent visits 14 day diary		501
8	Individually-kept diary, frequent visits 14 day diary		503
			4,025

Notes: Frequent visits entailed daily visits by the local assistant and visits every other day by the survey enumerator for the duration of the 2-week diary. Infrequent visits entail 3 visits: to deliver the diary (day 1), to pick up week 1 diary and drop off week 2 diary (day 8), and to pick up week 2 diary (day 15). Households assigned to the infrequent diary but who had no literate members (about 18 percent of the sub-sample) were visited every other day by the local assistant and the enumerator.

Non-food items are divided into two groups based on frequency of purchase. Frequently purchased items (charcoal, firewood, kerosene/paraffin, matches, candles, lighters, laundry soap, toilet soap, cigarettes, tobacco, cell phone and internet, transport) were collected by 14-day recall for modules 1-5 and in the 14-day diary for modules 6-8. Non-frequent non-food items (utilities, durables, clothing, health, education, contributions, and other; housing is excluded) are collected by recall identically across all modules at the end of the interview (and at the end of the 2-week period for the diaries) and over the identical one or 12-month reference period, depending on the item in question.

^a These 17 foods accounted for 77% of food consumption expenditure in the previous Household Budget Survey for Tanzania so the measured value of food consumption from this module is scaled up by 1/0.77. This module is informative about rapid surveys and other abbreviated consumption modules that occur when more comprehensive consumption surveys are not fielded, and as such it depends on the results from such comprehensive surveys so as to scale up to an estimate of total food consumption.

Table 2. Tests for mean-reverting measurement error in log consumption

	Mean	Ratio to benchmark	Variance	Ratio to benchmark	$\hat{\lambda}$ (S.E.)	<i>t</i> -test for correlated errors	<i>Split-Sample Tests</i>		<i>p</i> -value for equal	Elasticity for poly- nomial form of Eqn (1)
		$\frac{E(\ln \bar{x}_k)}{E(\ln \bar{x}_8)}$		$\frac{\text{var}(\ln \bar{x}_k)}{\text{var}(\ln \bar{x}_8)}$		$H_0: \lambda = 1$ $H_1: \lambda < 1$ $p = \Pr(t < \hat{t})$	Rural $\hat{\lambda}$ (S.E.)	Urban $\hat{\lambda}$ (S.E.)	$\hat{\lambda}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1. Long 14 day	14.104	0.987	0.350	1.081	0.569 (0.068) ^a	$p=0.000$	0.211 (0.097)	0.641 (0.121)	0.005	0.533 ^b
2. Long 7 day	14.225	0.996	0.337	1.040	0.596 (0.064)	$p=0.000$	0.231 (0.089)	0.533 (0.107)	0.027	0.510 ^b
3. Subset 7 day	14.195	0.994	0.320	0.988	0.535 (0.065)	$p=0.000$	0.124 (0.086)	0.576 (0.116)	0.001	0.601 ^b
4. Collapsed 7 day	14.039	0.983	0.343	1.060	0.583 (0.066)	$p=0.000$	0.131 (0.084)	0.753 (0.125)	0.000	0.608 ^b
5. Long usual month	14.084	0.986	0.423	1.307	0.662 (0.072)	$p=0.000$	0.195 (0.093)	0.715 (0.133)	0.001	0.640 ^b
6. HH diary frequent	14.128	0.989	0.289	0.891	0.494 (0.062)	$p=0.000$	0.174 (0.095)	0.673 (0.107)	0.001	0.413 ^b
7. HH diary infrequent	14.155	0.991	0.269	0.832	0.422 (0.063)	$p=0.000$	0.210 (0.097)	0.629 (0.110)	0.004	0.387 ^c
8. Benchmark (indiv, freq)	14.283	1.000	0.324	1.000						

Note: The $\hat{\lambda}$ are from separate regressions for each module, where the independent variable is village-averaged log annualized total household consumption from the individually-kept, frequent visit diary (the benchmark). $N=168$ (110 rural and 58 urban locations).

^a Standard errors in parenthesis.

^b Best fitting polynomial is a cubic (according to sequential ANOVA).

^c Best fitting polynomial is a quadratic (according to sequential ANOVA).

Table 3: OLS coefficient estimates and hypothesis test results for the food Engel curves^a

	Consumption Survey Module Number:							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ln per capita cons	-0.039*** (0.008)	-0.068*** (0.009)	-0.083*** (0.010)	-0.060*** (0.010)	-0.056*** (0.010)	-0.059*** (0.010)	-0.059*** (0.010)	-0.026*** (0.010)
ln household size	-0.072*** (0.013)	-0.043*** (0.013)	-0.046*** (0.015)	-0.058*** (0.014)	-0.048*** (0.016)	-0.050*** (0.014)	-0.044*** (0.013)	-0.037** (0.015)
Observations	503	504	504	504	504	502	501	503
Adjusted- R^2	0.360	0.332	0.438	0.369	0.318	0.430	0.351	0.386
p -value $H_{0:\hat{\beta}_k} = \hat{\beta}_8$	0.310	0.002	0.000	0.016	0.032	0.019	0.016	0.002 ^b
p -value $H_{0:\hat{\gamma}_k} = \hat{\gamma}_8$	0.078	0.763	0.667	0.324	0.632	0.553	0.731	0.773 ^b

Notes:

^aThe unreported covariates are the share of children aged less than six, children aged six to fifteen, and elders aged over 65, the age, school years, gender and marital status of the household head, and month and district fixed effects.

^bJoint test that the coefficients are equal across all eight columns.

Standard errors in parentheses; *, ** and *** denote statistical significance at 10%, 5% and 1% level. The consumption modules that match the numbers in the column headings are:

- 1 Long list (58 items), 14 day
- 2 Long list (58 items), 7 day
- 3 Scaled subset list (17 items), 7 day
- 4 Collapsed list (11 items), 7 day
- 5 Usual month (58 items)
- 6 Household diary, Frequent
- 7 Household diary, Infrequent
- 8 Individually-kept diary, Frequent