Destruction and distress: using a quasi-experiment to show the effects of the September 11 attacks on mental well-being in the United Kingdom

Robert Metcalfe University of Oxford

Nattavudh Powdthavee Nanyang Technological University

Paul Dolan London School of Economics

October, 2010

Abstract

Using a longitudinal household panel dataset in the United Kingdom, where a significant proportion of the interviews are conducted in September each year, we are able to show that the attacks of September 11 resulted in lower levels of subjective well-being for those interviewed after that date in 2001 compared to those interviewed before it. This quasi-experiment provides one of the first examples of the impact of a terrorist attack in one country on well-being in another country.

Keywords: terrorism; September 11; subjective well-being.

JEL: H56; I31.

Acknowledgements: We thank three anonymous referees for their excellent comments. The British Household Panel Survey data were made available through the UK Data Archive. The data were originally collected by the ESRC Research Centre on Micro-social Change at the University of Essex, now incorporated within the Institute for Social and Economic Research. Neither the original collectors of the data nor the Archive bear any responsibility for the analyses or interpretations presented here.

Terrorism is a major negative externality (Frey, 2004). Some costs of terrorism are very direct and relatively easy to measure, such as the value of lives lost, reduction in national output, etc., whilst others are more indirect and much more difficult to measure, such as increased fear and anxiety. One of the most recent and well-known terrorist attacks is the attacks of September 11th 2001 in the United States. It has been shown that the attacks had detrimental effects to the economy in the United States, particularly in the New York region (Chernick & Haughwout, 2006) as well as intangible psychological costs (Galea et al., 2002; Schlenger et al., 2002).

The indirect effects of terrorism, which might be large in their own right, could extend beyond national borders as they dominate media coverage (Eisensee & Stromberg, 2007). It is very difficult to identify the causal effects of terrorist attacks on individuals, regions or countries since there are sometimes no good comparable counterfactuals. As a result of this, one way of gauging the well-being consequences and therefore value of the indirect negative externalities would be through a stated preference study, which would elicit a direct willingness-to-pay (WTP) for a reduction in the risk of a terrorist attack. Smith et al (2009) found that U.S. individuals have a positive WTP for an anti-terrorism defence policy (between $100 to $220 annually per household). Similarly, using a random utility model, Viscusi (2009) found that reductions in deaths from terrorism have a value almost twice as great as reductions in deaths from natural disasters, suggesting a large premium for dread risk. To identify the international negative spillovers of the 9/11 attacks in the UK, for example, we could ask the United Kingdom population how much they are willing to pay to eliminate the risk of terrorist attacks in another country, such as the United States. This hypothetical WTP approach however has already been heavily debated and contested (see Mitchell and Carson, 1989; Diamond and Hausman, 1993; Ariely et al, 2003), and is

only one method of valuing non-market goods.

Another way is through people's experienced utility (Kahneman et al, 1997; Dolan and Kahneman, 2008), or what we describe as subjective well-being (SWB). This approach has already been used to determine the impact of income (Stevenson & Wolfers, 2008), unemployment (Clark & Oswald, 1994), and public policies (Gruber & Mullainathan, 2005) on SWB. It has also been used to determine the impact that non-market goods have on SWB (e.g. van Praag and Baarsma, 2005; Dolan and Metcalfe, 2008; Luechinger, 2009). The use of SWB has shown to be a valid and reliable indicator of well-being (see Diener et al, 1999; Krueger and Schkade, 2008), especially since evolution may have created the sensation of happiness exactly in order to affect our behaviour (Rayo and Becker 2007).

This study presents the first causal evidence of an international spillover of terrorism using people's SWB. We use the British Household Panel Survey (BHPS) to examine how the 9/11 attacks in the United States had detrimental effects on the SWB of residents in the United Kingdom. The BHPS allows us to examine the 9/11 attacks in a quasi-experimental setting – the interviews are randomly completed in the first few weeks, which is the novel feature of our study. The BHPS is administered annually, with a significant proportion of the surveys taking place during September in a random manner. So comparing the SWB levels of the United Kingdom population in the periods before and after the 11th September in 2001, and comparing this to the same residents in 2000, provides us with a novel and powerful quasi-experiment.

We find that the 9/11 terrorist attacks lowers the levels of SWB of those United Kingdom residents who answered the survey after the 11th of September in 2001. This effect is large

and robust to a number of alternative specifications and samples. By controlling for balanced/unbalanced panels, attrition, the ordinal nature of subjective well-being data, duration after attacks, seasonality, and household structure, we still find that the attacks had a significant negative impact on subjective well-being in the United Kingdom. The negative effects lasted until the end of November in 2001 and then dissipated. The magnitude of the average treatment effect is at least equal to half the size of the negative impacts of unemployment. The findings provided in this study are, to the best of our knowledge, the first evidence of its kind on an international spillover effect of terrorism.

1. Background

Terrorism and terror attacks have long been a major international problem, with potentially serious consequences for human welfare (Frey et al., 2007). The attacks of September 11th, 2001, were one of the most prominent acts of terrorism in recent times but just what are the consequences of such attacks? Economists use the underlying exogeniety of terrorist attacks as a way to establish the causal relationship from those attacks to various economic outcomes, such as tourism (Enders et al., 1992), national output (Abadie & Gardeazabal, 2002; Eckstein & Tsiddon, 2004), net foreign direct investment (Abadie & Gardeazabal, 2008) and urban expansion (Blomberg & Sheppard, 2007). Terrorism however only directly affects a small fraction of the capital stock (Becker & Murphy, 2001), and ~~that~~ it does not affect all economic outcomes (e.g. Glaeser & Shapiro, 2002 find that terrorism has not altered the urban composition, i.e. people still want to live and work in the attacked areas after being attacked). The well-being consequences of terrorism have also been studied in terms of the birth weight of babies in areas with a higher concentration of land mines, where the causal mechanism is thought to be the effects on the stress of mothers during pregnancy, especially during the third trimester (Camacho, 2008).

The terrorist attacks of September 11, 2001, have stimulated quite a bit of research in their own right. For example, there is now evidence to suggest that the attacks had a detrimental effect on the financial market (Chen & Siems, 2004; Straetmans et al., 2008) and New York's fiscal position (Dolfman & Wasser, 2004; Chernick & Haughwout, 2006). It has also been shown that the 9/11 attacks reduced the demand for air travel (Blunk et al., 2006; Blalock et al., 2007), with estimates ranging from $14 to $43billion a year (Santos and Haimes, 2004) to $214 to $420 billion a year (Gordon et al., 2007). There was also a significant increase in the number of fatal traffic accidents after 9/11 (Gigerenzer, 2004; Su et al., 2009), which has been found for other terrorist attacks (Stecklov & Goldstein, 2004).

In terms of the intangible effects of 9/11, it has been found that survivors from damaged buildings of the attacks reported substantial physical and psychological health problems three years after the event (Brackbill et al., 2006). Post-traumatic stress disorder (PTSD) has been shown to be associated with direct exposure to the 9/11 attacks and the prevalence of PTSD in the New York City metropolitan area was substantially higher than elsewhere in the country (Galea et al., 2002; Schlenger et al., 2002) – although there were no comparisons of PTSD before the attacks. Eidelson et al. (2003) find a significant increase in the amount of work – in terms of the number of clients – received by psychologists working closest to Ground Zero compared to those received by their colleagues working elsewhere in the country.

It seems that the intangible effects of 9/11 were felt elsewhere in the United States. For example, PTSD was not limited to those who experienced the 9/11 attacks directly (Silver et al., 2002), although the actual levels of stress outside of New York are disputed (Schlenger

et al, 2002). In a small sample from Wisconsin, Krueger (2007) found that 9/11 increased sadness temporarily and decreased enthusiasm for at least seven days after the attacks. In a nationally representative sample of Americans, Lerner et al. (2003) found a heightening level of fear and anger amongst the United States population following 9/11. More recently, the terrorist attacks in London in 2005 (which killed 52 people) have been shown to have negative effects on stress and have altered travel behaviour (Rubin et al, 2005) and criminal behaviour through extra policing (Draca et al., forthcoming).

Despite these and a range of other studies, virtually no attempts have been made to determine the effects of the attacks on the SWB of those outside of the attacked country. In addition to this, this paper helps contribute to a small number of studies in the SWB literature that provides a causal estimate of an event or an experience on SWB. For instance, Frijters et al (2004) report the exogenous impact German re-unification had on East Germans SWB. Oreopoulos (2007) presents the causal effect of a changing in the UK compulsory schooling law on life satisfaction. Powdthavee (2010) estimates the casual effect of income on life satisfaction using exogenous variation of whether or not the respondent's payslip is shown to the interviewer as an instrumental variable. Overall, many SWB studies do not have or use exogenous variation in their variable of interest.

2. Data and empirical strategy

2.1 Data

This study examines the effects of 9/11 on the level of mental distress of those living in the United Kingdom. This study has two main strengths. First, we use a large longitudinal dataset, consisting of approximately 10,000 individuals, which provide us with strong statistical power to discern patterns whist controlling for individual

heterogeneity and underlying trends. Second, 9/11 acts as an exogenous shock to the randomised sampled population, which provides us with a very powerful quasi-experiment.

The British Household Panel Survey (BHPS) is nationally representative of British households, and is conducted from the month of September of each year (started in 1991). Respondents are interviewed in successive waves and the sample has remained representative of the British population since the early 1990s. For the study to be thought of a quasi-experiment, the timing of terrorist attacks need to be exogenous and largely randomly assigned in terms of the BHPS interviews.[1] The 9/11 attacks were clearly exogenous to the survey since many respondents are interviewed in September each year but the date in September in which they are interviewed is random.

The measure of mental distress used in this analysis is the twelve items from the negative affect scale of the General Health Questionnaire (Goldberg, 1978). Respondents are asked how often (on a four point category scale) over the past few weeks they: (i) had lost sleep over worry; (ii) felt constantly under strain; (iii) felt they could not overcome difficulties; (iv) been feeling unhappy and depressed; (v) been losing confidence; (vi) been feeling like a worthless person; (vii) were playing a useful part in things; (viii) felt capable of making decisions; (ix) been able to enjoy day-to-day activities; (x) been able to concentrate; (xi) been able to face up to problems; and (xii) been feeling reasonably happy. The number of times a person places himself or herself in the top two categories was given a one, and then all twelve questions were added together to produce what is known as a

[1] Extract from personal correspondence with ISER individual: "*The majority of respondents are interviewed in the six - eight weeks from Sept 1st. There is then a rather long tail with diminishing numbers that go into the new year. This is mainly due to refusal conversion, tracing and reissues to field … In terms of contact, the instruction is to contact everyone within the first three weeks but this falls down if people are away on holiday, have moved and need to be reallocated or traced or are non-contactable for some other reason (in hospital etc) … Given the interview distribution I expect that most are called on within 4 weeks as many interviewers set up their appointments in advance.*"

Caseness measure of mental distress, with the highest level of distress value scores 12 and minimum distress level scores zero. This composite rating is a good proxy for the transient component of moods (Watson and Clark, 1984) and has been used as a measure of SWB in recent studies by economists (Blanchflower & Oswald, 2008; Clark, 2003; Clark & Etilé, 2002; Gardner & Oswald, 2007; Jones & Wildman, 2008) and to value intangible goods (e.g. Oswald & Powdthavee, 2008).

We consider three different subsamples for our analysis. The first unbalanced subsample (Sample 1) includes all adult individuals who were interviewed between January 2000 and December 2001 (i.e. two overlapping BHPS waves). This initial subsample includes 26,582 observations (15,944 individuals) of which 10,995 observations are in 2000 and 15,587 observations are in 2001. The additional 4,592 observations in 2001 come mainly from the booster sample included in the BHPS from Northern Ireland. In addition to this, because the interview dates are random, some individuals ended up having been interviewed twice in the same calendar year (for example person A may have been interviewed for Wave 10 in January 2001 and then again in December 2001 for Wave 11). For this reason, we decided to drop these small numbers of individuals (approximately 5% of the total sample) from our analysis, but this does not impact on the following results.

For the year 2001, our analysis separates the initial sample into two groups – (i) the pre-9/11 subsample (or the control group), which consists of individuals who were surveyed between 1st January 2001 and 10th September 2001, and (ii) the post-9/11 subsample, which consists of individuals who were surveyed between 12th September 2001 and 31st December 2001. The two groups are then tracked back to the smaller sample in year 2000 and given the same ID code (0 = pre-9/11/2001, 1 = post-9/11/2001), regardless of whether they were also

interviewed in the same period of time in 2000 as they would be in 2001. So, for example, if person B was interviewed post-9/11 in Wave 11 of the BHPS (e.g., September 15th, 2001) but pre-9/11 in Wave 10 (e.g., September 5th 2000), he will still be assigned a value of 1 in the "post-9/11" dummy variable for both years in the panel.

For further regression analysis and to obtain more refined estimates of the 9/11 attacks on SWB, we consider the three-year period before and after the September 11th attacks (1998-2004). This gives us another unbalanced subsample (Sample 2) that consists of 78,348 observations (16,333 individuals). And finally, a smaller subsample of Sample 2 is considered for the balanced panel estimation – named Sample 3. This consists of 37,149 observations, or 5,307 individuals who were present in all the BHPS waves from 1998-2004. These estimates are more refined since we can examine any selection effects in the sample.

So we have three samples that we will use for the analysis: Sample 1 – unbalanced subsample (Sample 1) includes all adult individuals who were interviewed between January 2000 and December 2001 (15,944 individuals); Sample 2 – seven year unbalanced panel, which is three-years before and after the September 11th attacks (1998-2004) (16,333 individuals); Sample 3 – seven year balanced panel, which is three-years before and after the September 11th attacks (1998-2004) (5,307 individuals). Descriptive statistics of Samples 1, 2 and 3 are shown in Appendix A. Individuals in the post-9/11 sample tend to report slightly lower levels of mental distress, are richer, have more children, and are likely to be cohabiting with a partner than those in the pre-9/11 sample. The reason why this is the case is unknown, but it is therefore important to control for these characteristics.

2.2. Accounting for selection bias

We acknowledge that there may well be some selection bias involved in moving from Sample 2 to Sample 3, as a number of contemporaneous shocks may have simultaneously affected the duration of the selected sample remaining in the BHPS and their SWB. For this reason, a balanced panel estimate may not truly reflect the causal impact of the 9/11 attacks on the level of mental distress of the total population. For instance, one might postulate that that the most affected individuals of the 9/11 attacks may not want to be interviewed post-9/11 in 2001, which could bias the SWB responses post-9/11 downwards, i.e. the impact would be less negative if we do not account for this attrition.

To correct for any selection bias from moving from Sample 2 to Sample 3, we compute a Mills ratio using a selection variable that equals one if the individual is observed in every year between 1997 and 2004, and zero otherwise as our dependent variable in the selection equation. This balanced panel selection equation is estimated on Sample 2, as shown in Appendix B, as a function of gender, education (2 dummies), age, age-squared, marital status (4 dummies), number of children aged under 16, labour force status (9 dummies), household income, year dummies (6 dummies), regional dummies (19 dummies), and the interviewer's observation on whether the respondent is cooperative at giving the interview or not. This last variable is used to satisfy the exclusion restrictions, which is possible as the attrition rate should be correlated with the interviewer's perception about how cooperative the interviewees are during the interview (i.e. people who do not seem to want to cooperate at t are likely to drop out comes t+1), but should not be correlated with the timing of the 9/11 attacks.[2]

[2] To clarify, the number of interviews after the 9/11 attacks did not slow down as a result of the attacks in comparison to any other year, and the T-tests of the relative differences in the background variables between the pre-9/11 and the post-9/11 groups were the same – in terms of statistical significances - in the years before 9/11 as well as in the years after 9/11.

2.3. Empirical strategy

Let D_{it} be the distress level of individual i at time t, "Post 9/11" be a dummy variable denoting 1 if the individual was interviewed post-9/11 in 2001 and 0 otherwise (see section 3.1 for details), "Year = 2001" be a dummy variable representing the year of the 9/11 attacks, u_i is an individual fixed effects assuming to be uncorrelated with the timing of the 9/11 attacks, and ε_{it} is a time-varying random shock. Here, we consider the following simple difference-in-differences (D-i-D) approach to be estimated on Sample 1 in order to approximate the causal effect of the 9/11 attacks on mental distress of the UK population:

$$D_{it} = \alpha + \beta_1 \text{Post9/11}_{it} + \beta_2 (\text{Year} = 2001)_t + \beta_3 (\text{Post9/11}_{it} \times (\text{Year} = 2001)_t) + u_i + \varepsilon_{it}, \quad (1)$$

Given that the 9/11 attacks in 2001 were unanticipated, and assuming that in the absence of the attacks D_{it} would have changed identically in the pre- and post-9/11 groups between 2000 and 2001, the parameter β_3 will then represent the causal effect of the 9/11 attacks on the mental distress of those interviewed between 12th September 2001 and 31st December 2001. More formally, in the absence of treatment, β_3 would be statistically indifferent from zero, i.e. the pre- and post-9/11 SWB is the same in 2001 as in any other year (see Meyer, 1995). In this case, an unbiased estimate of β_3 can be obtained by D-i-D as:

$$\hat{\beta}_3 = \Delta\overline{D}^{Post9/11}_{2000\text{-}01} - \Delta\overline{D}^{Pre9/11}_{2000\text{-}01}$$
$$= \overline{D}^{Post9/11}_{2001} - \overline{D}^{Post9/11}_{2000} - (\overline{D}^{Pre9/11}_{2001} - \overline{D}^{Pre9/11}_{2000}) \quad (2)$$

Note that, for robustness checks, this approach can be extended to include multiple time periods and multiple treatment groups, i.e. by applying (1) to Samples 2 and 3. We can then estimate β_3 by applying either OLS with clustering on the personal identification, a random-effects model, or a fixed-effects model on (1). An ordered probit with random effects can also be applied on (1) to account for the fact that the dependent variable is ordinally measured rather cardinally measured. Finally, further specifications can also be introduced to (1) in order to account for the multilevel structure of the data (i.e. two people can be living in the same household) and serial correlations in the panel data, both of which will be described in more details in the results section.

3. Results

Did the 9/11 terrorist attacks in the United States raise the distress level for the United Kingdom population in 2001? Table 1 takes a first look at this question by estimating a D-i-D model using OLS, generalised least squares with random effects (RE), fixed-effects (FE) model, and an ordered probit with random effects on the distress level of people surveyed in the BHPS. A naïve estimation for only 2001 is unlikely to produce the correct estimate of the September 11th attacks on mental distress because there is an increase in mental distress (or more generally, a decrease in the SWB) each year as we go beyond September, peaking in November.

We can see from Table 1 that the interaction coefficients between "Post-9/11" and "Year=2001" are positive and statistically significant at least at the 5% level across the three columns. The signs and qualitative trade-offs between coefficients are very similar across the four models, suggesting that there is little difference to the interpretation of the results whether one assumes cardinality or ordinality in the mental distress data (see Ferrer-i-Carbonell & Frijters, 2004). For OLS, RE and FE models, the point estimates on the effect of the 9/11 attacks on mental distress are between 0.17-0.27, which is approximately 7% of the standard deviation of the GHQ responses. This implies that whilst there is a slight decrease in the level of mental distress between 2000 and 2001 for the control group (i.e. the pre-9/11), the post-9/11 group actually experienced a statistically significant rise in the distress level between 2000 and 2001: the sum of the coefficients between "Year=2001" and "Post-9/11 x Year=2001" in the generalised least squares with random effects equation is 0.061, with a statistically well-defined standard error of 0.031.

So we find a negative impact of the 9/11 attacks on SWB as measured by the GHQ. We have already shown that the GHQ is a composite measure of twelve subjective states, so knowing what states are impacted upon by the attacks is very worthwhile. Table 2 uses Sample 1 but replaces the overall GHQ with the twelve individual measures (similar to the analysis of Huppert & Whittington, 2003). Table 2 shows that there are four main domains of the GHQ that drive the impact of the attacks on SWB: less capability to make decisions (GHQD); less enjoyment from day-to-day activities (GHQG); less ability to face problems (GHQH); and less frequency of feeling happy (GHQL). The 9/11 attacks had the largest impact on the more affective measures, i.e. enjoyment and happiness, and these are the measures that are driving the GHQ. These are the affective states that are more alike the SWB measures used in the literature, such as life evaluation and moment-to-moment experience (Kahneman et al, 2004).

Nonetheless, for completeness, it is important to check the underlying trends of mental distress between pre- and post-9/11 across many of the years (see a critique by Bertrand et al., 2004). In order to satisfy one of the D-i-D conditions, it is pertinent for researchers to show that, in absence of treatment, the differences between treatment and control group would have remained constant over time (Meyer, 1995). We can check whether this is the case for the 9/11 attacks by plotting the distress trends for the control and the treated groups prior to 2001. Here, a three year period before and three year period after the event (i.e. Sample 2 – unbalanced seven year panel) is chosen to generate the plot, although similar patterns (but with significantly smaller N) can be obtained with longer leads and lags, and with the balanced panel.

We can see from Figure 1 that the average distress levels for both pre and post-9/11 groups follow a very similar trend in the years that precede 2001. The trend however diverges in the year of the September 11 attacks. That is, there is a noticeable increase in the average distress levels of those interviewed post-9/11 from 2000 to 2001, which is consistent with the estimated average treatment effects obtained in Table 1[3].

Table 3 incorporates multiple time periods and treated groups into the analysis by estimating (1) on Sample 2, i.e. individuals observed between 1998 and 2004. Applying the generalised least squares with random effects model (RE) on the seven-year unbalanced sample (1998-2004) yields in Column 1 of Table 3 a positive and statistically significant average treatment effect at the 5% level. This column adjusts for seasonality by including the interviewed

[3] Although it is perhaps worth noting here of another divergence in the trends again in 2003, which indicates that there may well be other unobserved third variables that have not been controlled for pre- and post-2001. The robustness checks section will attempt to deal with this issue.

month dummies into the regression. This is to allow for the possibility that individuals in the control group may have done their interviews pre- and post-2001 in the doom and gloom wintry months, whilst those in the treated group may have done their interviews in the relatively more cheerful autumn of the same year. In addition to the seasonal dummies, Column 1 also controls for the standard subjective well-being control variables (Clark et al., 2008; Dolan et al., 2008), including gender, age, age-squared, log of real household income, as well as dummies for marital status, labour force status, education, number of children, and regions. With this full specification[4], the average treatment effect ("Post-9/11 x Year=2001") continues to be positive and statistically significant at the 5% level; the coefficient on the average treatment effect is approximately 0.2, with a well-defined standard error of 0.082. All of the coefficients on other average treatment effects are now insignificantly different from zero. Estimating FE also yields a similar average treatment effect of 0.2.

So far in our analysis, we have been using the individual as the primary unit of observation. One objection to this is that some observational units in the BHPS are likely to share a household, which may have efficiency implications for the estimates reported given the correlation in the unobserved factors determining subjective well-being within households (see, e.g., Powdthavee & Vignoles, 2008; Powdthavee, 2009). To account for the hierarchical structure of how the data is constructed, equation (1) is expanded to include the unobserved group effect at the household level as follows:

$$D_{iht} = \alpha + \beta_1 \text{Post9/11}_{iht} + \beta_2 (\text{Year}=2001)_t + \beta_3 (\text{Post9/11}_{iht} \times (\text{Year}=2001)_t) + u_i + v_h + \varepsilon_{iht}, \quad (3)$$

[4] See Appendix C for Column 2's other results.

where the subscript h denotes household as the secondary unit of observation, and v_h represents the unobserved group effect (or residual) for household. Typically, the random parameter v_h is assumed to be normally distributed: $v_h \sim N(0,\sigma_h^2)$. We can then estimate (3) using a multilevel model (ML) (see Goldstein, 2003). Nevertheless, ML (Column 3 of Table 3) continues to produce an average treatment effect ("Post-9/11 x Year=2001") that is both positive and statistically well-determined. The effect of the 9/11 attacks on mental distress continues to be around 0.2 on the 0-12 GHQ Caseness scale.

The last column of Table 3 explores the possibility that the estimation of equation (1) is subject to a serial correlation problem. According to Bertrand et al. (2004), the most commonly used dependent variables in D-i-D studies are typically highly positively serially correlated, which could potentially severely upward bias the standard error of the average treatment effect (β_3 in our case). To try and account for the serial correlation issue, we first estimate in Column 4 of Table 3 a generalised least squares with random effects that also accounts for an AR(1) disturbance. Allowing for a short-term serial correlation does not hugely change our results in anyway; the estimated coefficient on the average treatment effect for 2001 continues to be positive and statistically significant at approximately 0.2, with a standard error of 0.078.

Nonetheless, as found by Bertrand et al. (2004), allowing only for an AR(1) disturbance in the estimation of (1) may not be enough to counter the serial correlation problem. To solve this problem, one solution advised by Bertrand et al. is to ignore the time-series information altogether when computing the standard error. In other words, all we need to do here is to simply average the data in the three years before the date of the 9/11 attacks, i.e. from 1998-

2000, and rerun equation (1) on this averaged outcome variable (pre-2001) and the distress level reported in 2001 in a panel of time length equal to two. Running a generalised least squares with random effects on this modified data produces a coefficient on the average treatment effect for 2001 of 0.193, with a statistically well-determined standard error of 0.066 (column 5 of Table 3). What this implies is that our results continue to hold even when serial correlations in the dependent variable have all but been taken into account to the best of our ability.

Given the robustness of this result to alternative specifications, we can now look into what type of individual was most impacted by the 9/11 attacks. Table 4 recalculates the previous table's random effects estimation by age and gender group. Columns 1 and 2 split the sample by female and male respectively. It is clear that the 9/11 attacks had a detrimental impact on SWB for females, but the detriment for men is not significantly different. This sheds some light on the asymmetrical effects of large-scale terrorist events for gender. Columns 3 and 4 split the sample by age (i.e. under and over 35s), and it is clear that the attacks had a larger impact on the older rather than the younger individuals in the sample.

As usual, one concern is that attrition in the panel could be problematic. People may refuse to continue to participate in the rest of the panel if they are personally affected by the 9/11 attacks (e.g. family involved in the attacks etc.), leading to an underestimation of the true impact of the terrorist attacks on people's well-being. This is that people might leave the panel after the 2001 wave, and implies is that people who are relatively more adversely affected by the 9/11 attacks may have been non-randomly left out from our unbalanced sample, leading to an overestimation of the results. To be sure that the results are not being driven by individuals in the unbalanced sample who are in the panel only briefly, equation (1) is rerun in Table 5 on

a smaller balanced panel (i.e. Sample 3) using both RE and FE models. Note that, whilst the other year treatment effects continue to be statistically insignificantly different from zero in both columns, there is indeed a notable increase (more than 100%) in the estimated average treatment effect ("Post-9/11 x Year=2001"). To our surprise, the estimated effect is larger in the balanced panel in comparison to the unbalanced panel; the estimated coefficient in the RE model is now 0.435, with a standard error of 0.137 and in the FE model it is 0.44 with a standard error of 0.14. This result may be due to the significant decrease in the control group's sample size (a 50% decrease from over a thousand observations in each case, leaving just over six hundred observations in each year of the balanced panel). It is also these individuals who happened to experience an unanticipated sharp drop in mental distress in 2001, thereby increasing the estimated average treatment effect from 0.20 to around 0.43. As a result, the balanced panel should be treated with caution. Nevertheless, the results obtained in the balanced panel do control for panel attrition and seem to be consistent with what have been obtained previously in the unbalanced panel, thus further supporting our case. The inversed Mills ratio (obtained from the selection equation in appendix B) is also positive though statistically insignificant, thus suggesting that we need not worry about the potential correlation between the attrition rate and mental distress in our balanced panel estimation.

Table 5 moves on to explore whether the effect of the 9/11 attacks heightened or dissipated in the few months that followed the event. To do this, we split the post-9/11 group into four sub-samples: (i) 12th September 2001-20th September 2001, (ii) 21st September 2001-30th September, (iii) 1st October 2001-31st October 2001, (iv) 1st November 2001-30th November 2001, and (v) 1st December 2001-31st December 2001, and rerun them using both RE and FE models on the balanced panel (Sample 3). We will just summarise the FE results here although we find the same result with RE. It seems that the greatest impact of the 9/11 attacks

happened at the end of September rather than just after the event, that is the average treatment effect (ATE) was 0.52 in late September but 0.33 in mid September. This is consistent with the temporal aspect of the GHQ in that it asks for your self-report 'recently' and not 'currently'. The impact of the attacks in October and November for this sample are significant (ATE was 0.48 and 0.49 respectively), but by December the impact of the attacks seem not to have an impact on people's SWB.

So, how large was the effect of the 9/11 attacks on mental distress of the United Kingdom population? Taking a conservative estimate of the ATE to be 0.2, this is roughly one-third the size of the negative impact of widowhood; it is approximately 16% the size of the mental distress brought about by unemployment. If we use the balanced panel estimates, which have no attrition but a lower sample size, these estimates would increase by at least double. This therefore clearly demonstrates the large impact of large negative externalities have on the welfare of individuals in other countries.

4. Discussion

This study has shown that the 9/11 attacks in the United States lowered the subjective well-being of United Kingdom residents – by a GHQ well-being score of approximately 0.2-0.4. Comparing this magnitude with other life events within our data is difficult since many events, such as marriage or being unemployed, are not exogenous. Notwithstanding this, the magnitude of the 9/11 effect is potentially worse than becoming divorced, and about at least half the effect of being unemployed in the same sample using the same methods. These are significant and robust effects. This research goes some way towards demonstrating that the fear and psychological cost induced by terrorism is substantial and might

greatly exceed the discounted physical harm (Sunstein, 2003; Becker and Rubinstein, 2004). This is due to the fact that 'dread' makes up a significant part of the risks from terrorism (Viscusi, 2009), and this especially true given that recent media coverage has been dominated by the 9/11 attacks (Eisensee and Stromberg, 2007).

Whatever the precise scale the impact of 9/11 across the United Kingdom population, it is possible that individuals in the UK were affected by 9/11 because they believed that such events were more likely to happen in the United Kingdom in the near future, thereby increasing their fear and uncertainty. Given Krueger and Laitin's (2008) finding that terrorists are more likely to attack wealthy countries, it seems natural for individuals in other wealthy countries to be affected by terrorist attacks overseas. Indeed, the results from our study support the Caplin and Leahy (2001) model where the events that caused the initial fear and uncertainty took place in another country.

We can only speculate about such issues here as there has certainly been little discussion of the international spillover effects of security or terrorism. The United States Congress Joint Economic Committee (2002) has suggested that some of the largest costs of terrorism were the difficult to measure costs of added anxiety, stress, and psychological disorders associated with the increased threat of terrorism. This paper has shown that these costs may have also been very significant outside of the United States.

References

Abadie, A. and Gardeazabal, J. (2002). 'The economic cost of conflict: A case study of the Basque country.' *American Economic Review*, vol. 93, pp. 113-132.

Abadie, A. and Gardeazabal, J. (2008). 'Terrorism and the World Economy.' *European Economic Review*, vol. 52, pp. 1-27.

Ariely, D., Loewenstein, G. and Prelec, D. (2003). 'Coherent Arbitrariness: Stable Demand Curves without Stable Preferences.' *Quarterly Journal of Economics*, vol. 118, pp. 73-105.

Becker, G. and Murphy, K. (2001). 'Prosperity Will Rise Out of the Ashes.' *Wall Street Journal*, October 29.

Becker, G.S. and Rubinstein, Y. (2004). 'Fear and the Response to Terrorism: An Economic Analysis.' Unpublished paper, Department of Economics, University of Chicago.

Bertrand, M., Duflo, E. and Mullainathan, S. (2004). 'How Much Should We Trust Differences-in-Differences Estimates?' *Quarterly Journal of Economics*, vol. 119, pp. 249-275.

Blalock, G., Kadiyali, V. and Simon, D.H. (2007). 'The Impact of Post-9/11 Airport Security Measures on the Demand for Air Travel.' *Journal of Law and Economics*, vol. 50, pp. 731-755.

Blanchflower, D.G. and Oswald, A.J. (2008). 'Hypertension and happiness across nations.' *Journal of Health Economics*, vol. 27, pp. 218-233.

Blomberg, S.B. and Sheppard, S. (2007). 'The Impacts of Terrorism on Urban Form.' *Brookings-Wharton Papers on Urban Affairs*, pp. 257-290.

Blunk, S., Clark, D. and McGibany, J. (2006). 'Evaluating the Long-Run Impacts of the 9/11 Terrorist Attacks on US Domestic Airline Travel.' *Applied Economics*, vol. 38, pp. 363-370.

Brackbill, R.M., Thorpe, L., DiGrande, L., Perrin, M., Sapp, J., Wu, D., Campolucci, S., Walker, D., Cone, J., Pulliam, P., Thalji, L., Farfel, M. and Thomas, P. (2006). 'Surveillance for World Trade Center disaster health effects among survivors of collapsed and damaged buildings.' *Morbidity and mortality weekly report. CDC surveillance summaries*, vol. 55: pp. 1 -18.

Camacho, A. (2008). 'Stress and Birth Weight: Evidence from Terrorist Attacks.' *American Economic Review*, vol. 98, pp. 511-515.

Caplin, A. and Leahy, J. (2001). 'Psychological Expected Utility Theory and Anticipatory Feelings.' *Quarterly Journal of Economics*, vol. 116, pp. 55-79.

Chen, A.H. and Siems, T.F. (2004). 'The effects of terrorism on global capital markets.'

European Journal of Political Economy, vol. 20, pp. 349-366.

Chernick, H. and Haughwout, A.F. (2006). 'Tax Policy and the Fiscal Cost of Disasters: NY and 9/11.' *National Tax Journal*, vol. 59, pp. 561-577.

Clark, A.E. (2003). 'Unemployment as a Social Norm: Psychological Evidence from Panel Data.' *Journal of Labor Economics*, vol. 21, pp. 323-352.

Clark, A. and Etile, F. (2002). 'Do health changes affect smoking? Evidence from British panel data.' *Journal of Health Economics*, vol. 21, pp. 533-562.

Clark, A.E., Frijters, P. and Shields, M. (2008). 'Relative Income, Happiness and Utility: An Explanation for the Easterlin Paradox and Other Puzzles.' *Journal of Economic Literature*, vol. 46, pp. 95-144.

Clark, A.E. and Oswald, A. (1994). 'Unhappiness and Unemployment.' ECONOMIC JOURNAL, vol. 104, pp. 648-659.

Diamond, P. and Hausman, J. (1994). 'Contingent Valuation: Is Some Number Better than No Number?' *Journal of Economic Perspectives*, vol. 8, pp. 45-64.

Diener, E., Suh, E.M., Lucas, R.E. and Smith, H.L. (1999). 'Subjective well-being: Three decades of progress.' *Psychological Bulletin*, vol. 125, pp. 276-302.

Dolan, P. and Kahneman, D. (2008). 'Interpretations of utility and their implications for the valuation of health.' ECONOMIC JOUNRAL, vol. 118, pp. 215-234.

Dolan, P. and Metcalfe, R. (2008). 'Comparing Willingness-to-Pay and Subjective Well-Being in the Context of Non-Market Goods.' Centre for Economic Performance Discussion Paper No. 0890, London School of Economics.

Dolan, P., Peasgood, T. and White, M. (2008). 'Do we really know what makes us happy? A review of the economic literature on the factors associated with subjective well-being.' *Journal of Economic Psychology*, vol. 29, pp. 94-122.

Dolfman, M.L. and Wasser, S.F. (2004). '9/11 and the New York City Economy: A Borough- by-Borough Analysis.' *Monthly Labor Review*, vol. 127, pp. 3-33.

Draca, M., Machin, S. and Witt, R. (forthcoming). 'Panic on the Streets of London: Police, Crime and the July 2005 Terror Attacks.' *American Economic Review*.

Eckstein, Z. and Tsiddon, D. (2004). 'Macroeconomic consequences of terror: theory and the case of Israel.' *Journal of Monetary Economics*, vol. 51, pp. 971-1002.

Eidelson, R.J., D'Alessio, G.R. and Eidelson, J.I. (2003). 'The Impact of September 11 on Psychologists.' *Professional Psychology: Research and Practice*, vol. 34, pp. 144-150.

Eisensee, T. and Strömberg, D. (2007). 'News Droughts, News Floods, and U.S. Disaster Relief.' *Quarterly Journal of Economics*, vol. 122, pp. 693-728.

Enders, W., Sandler, T. and Parise, G.F. (1992). 'An Econometric Analysis of the Impact of Terrorism on Tourism.' *Kyklos*, vol. 45, pp. 531-554.

Ferrer-i-Carbonell, A. and Frijters, P. (2004). 'How Important is Methodology for the Estimates of the Determinants of Happiness?' ECONOMIC JOURNAL, vol. 114, pp. 641-659.

Frey, B.S. (2004). *Dealing With Terrorism: Stick Or Carrot?* Cheltenham: Edward Elgar.

Frey, B.S., Luechinger, S. and Stutzer, A. (2007). 'Calculating Tragedy: Assessing the Costs of Terrorism.' *Journal of Economic Surveys*, vol. 21, pp. 1-24.

Frijters, P., Haisken-DeNew, J.P. and Shields, M.A. (2004). 'Money does matter! Evidence from increasing real income and life satisfaction in East Germany following reunification.' *American Economic Review*, vol. 94, pp. 730-740.

Galea, S., Ahern, J., Resnick, H., Kilpatrick, D., Bucuvalas, M., Gold, J. and Vlahov, D. (2002). 'Psychological sequelae of the September 11 terrorist attacks in New York City.' *New England Journal of Medicine*, vol. 346, pp. 982-987.

Gardner, J. and Oswald, A.J. (2007). 'Money and psychological well-being: A longitudinal study of medium-sized lottery wins.' *Journal of Health Economics*, vol. 26, pp. 49-60.

Gigerenzer, G. (2004). 'Dread risk, September 11, and fatal traffic accidents.' *Psychological Science*, vol. 15, pp. 286–287.

Glaeser, E.L. and Shapiro, J. (2002). 'Cities and Warfare: The Impact of Terrorism on Urban Form.' *Journal of Urban Economics*, vol. 51, pp. 205-224.

Goldberg, D. (1978). *The manual of the General Health Questionnaire*. Windsor: NFER.

Goldberg, D., Oldehinkel, A. and Ormel, J. (1998). 'Why GHQ threshold varies from one place to another.' *Psychological Medicine*, vol. 28, pp. 915-921.

Goldstein, H. (2003). *Multilevel Statistical Models*. 3rd Edition. London: Arnold.

Gordon, P., Moore, J.E., Park, J.Y. and Richardson, H. (2007). 'The Economic Impacts of a Terrorist Attack on the U.S. Commercial Aviation System.' *Risk Analysis*, vol. 27, pp. 505-512.

Gruber, J.H. and Mullainathan, S. (2005). 'Do Cigarette Taxes Make Smokers Happier?' *The B.E. Journal of Economic Analysis & Policy*.

Huppert, F.A. and Whittington, J.E. (2003). 'Evidence for the Independence of Positive and Negative Well-Being: Implications for Quality of Life Assessment.' *British Journal of Health Psychology*, vol. 8, pp. 107-122.

Jones, A.M. and Wildman, J. (2008). 'Health, income and relative deprivation: Evidence

from the BHPS.' *Journal of Health Economics*, vol. 27, pp. 308-324.

Kahneman, D., Krueger, A.B., Schkade, D.A., Schwarz, N. and Stone, A.A. (2004). 'A Survey Method for Characterizing Daily Life Experience: The Day Reconstruction Method.' *Science*, vol. 306, pp. 1776.

Kahneman, D., Wakker, P.P. and Sarin, R. (1997). 'Back to Bentham? Explorations of Experienced Utility.' *Quarterly Journal of Economics*, vol. 112, pp. 375-405.

Krueger, A.B. (2007). *What makes a terrorist: economics and the roots of terrorism*. Princeton University Press: New Jersey.

Krueger, A.B. and Laitin, D.D. (2008). 'Kto Kogo?: A Cross-Country Study of the Origins and Targets of Terrorism.' In: Keefer, P., Loayza, N. (Eds), *Terrorism, Economic Development, and Political Openness*. Cambridge: Cambridge University Press.

Krueger, A.B. and Schkade, D.A. (2008). 'The reliability of subjective well-being measures.' *Journal of Public Economics*, vol. 92, pp. 1833-1845.

Lerner, J.S., Gonzalez, R.M., Small, D.A. and Fischhoff, B. (2003). 'Effects of fear and anger on perceived risks of terrorism: A national field experiment.' *Psychological Science*, vol. 14, pp. 144-150.

Luechinger, S. (2009). Valuing Air Quality Using the Life Satisfaction Approach. ECONOMIC JOURNAL, vol. 119, pp. 482-515.

Meyer, B.D. (1995). 'Natural and Quasi-Experiments in Economics.' *Journal of Business and Economic Statistics*, vol. 13, pp. 151-161.

Mitchell, R.C. and Carson, R. (1989). *Using Surveys to Value Public Goods*. Washington DC: RFF.

Oreopoulos, P. (2007). 'Do dropouts drop out too soon? Wealth, health and happiness from compulsory schooling.' *Journal of Public Economics*, vol. 91, pp. 2213-2229.

Oswald, A.J. and Powdthavee, N. (2008). 'Death, Happiness and the Calculation of Hedonic Damages.' *Journal of Legal Studies*, vol. 37, pp. S217-S252.

Powdthavee, N. and Vignoles, A. (2008). 'Mental health of parents and life satisfaction of children: a within-family analysis of intergenerational transmission of well-being.' *Social Indicators Research*, vol. 88, pp. 397-422.

Powdthavee, N. (2009). 'I can't smile without you: spousal correlation in life satisfaction.' *Journal of Economic Psychology*, vol. 30, pp. 675-689.

Powdthavee, N. (2010). 'How much does money really matter? Estimating the causal effect of income on happiness.' *Empirical Economics*, 39(1), pp77-92.

Rayo, L. and Becker, G S. (2007). 'Evolutionary Efficiency and Happiness.' *Journal of Political Economy*, vol. 115, pp. 302-37.

Rubin, G.J., Brewin, C.R., Greenberg, N., Simpson, J. and Wessely, S. (2005). 'Psychological and behavioural reactions to the bombings in London on 7 July 2005: cross sectional survey of a representative sample of Londoners.' *British Medical Journal*, vol. 331, pp. 606.

Santos, J.R. and Haimes, Y.Y. (2004). 'Modelling the Demand Reduction Input Output Inoperability due to Terrorism of Interconnected Infrastructures.' *Risk Analysis*, vol. 24, pp. 1437-1451.

Schlenger, W.E., Caddell, J.M., Ebert, L., Jordan, B.K., Rourke, K., Wilson, D., Thalji, L., Dennis, M., Fairbank, J.A. and Kulka, R.A. (2002). 'Psychological Reactions to Terrorist Attacks: Findings From the National Study of Americans' Reactions to September 11.' *Journal of the American Medical Association*, vol. 88, pp. 581-588.

Silver, R.C., Holman, A., McIntosh, D.N., Poulin, M. and Gil-Rivas, V. (2002). 'Nationwide Longitudinal Study of Psychological Responses to September 11.' *Journal of the American Medical Association*, vol. 288, pp. 1235-1244.

Smith, V.K., Mansfield, C. and Clayton, L. (2009). 'Valuing a Homeland Security Policy: Countermeasures for the Threats from Shoulder Mounted Missiles.' NBER Working Paper #14325.

Stecklov, G. and Goldstein, J.R. (2004). 'Terror attacks influence driving behavior in Israel.' *Proceedings of the National Academy of Sciences, U.S.A.*, vol. 101, pp. 14551-14556.

Straetmans, S., Verschoor, W.F.C. and Wolff, C. (2008). 'Extreme US Stock Market Fluctuation in the Wake of 9/11.' *Journal of Applied Econometrics*, vol. 23, pp. 17-42.

Stevenson, B. and Wolfers, J. (2008). 'Economic Growth and Subjective Well-Being: Reassessing the Easterlin Paradox.' *Brookings Papers on Economic Activity*, pp. 1-87.

Su, J.C., Tran, A.G., Wirtz, J.G., Lngteau, R.A. and Rothman, A.J. (2009). 'Driving Under the Influence (of Stress): Evidence of a Regional Increase in Improved Driving and Traffic Fatalities After the September 11 Terrorist Attacks.' *Psychological Science*, vol. 20, pp. 59-65.

Sunstein, C.R. (2003). 'Terrorism and Probability Neglect.' *Journal of Risk and Uncertainty*, vol. 26, pp. 121-136.

United States Congress, Joint Economic Committee. (2008). *The Economic Costs of Terrorism.* Washington DC: JEC.

van Praag, B.M.S. and Baarsma, B. (2005). 'Using Happiness Surveys to Value Intangibles: The Case of Airport Noise.' ECONOMIC JOURNAL, vol. 115, pp. 224-246.

Viscusi, W.K. (2009). 'Valuing Risks of Death from Terrorism and Natural Disasters.' Vanderbilt University Law School, Law and Economics, Working Paper Number 09-

08.

Watson, D. and Clark, L.A. (1984). 'Negative affectivity: The disposition to experience aversive emotional state.' *Psychological Bulletin*, vol. 96, pp. 465-490.

Table 1: Mental distress and the 9/11 attacks – unbalanced panel BHPS 2000-2001 (Sample 1)

Dependent variable: Mental distress	OLS	RE	OPROBIT-RE	FE
Post-9/11	-0.141	-0.173	-0.063	
	[0.080]	[0.075]*	[0.042]	
Year=2001	-0.109	-0.154	-0.048	-0.211
	[0.075]	[0.072]*	[0.039]	[0.076]**
Post-9/11 x Year=2001	**0.173**	**0.215**	**0.084**	**0.272**
	[0.082]*	**[0.079]****	**[0.042]***	**[0.083]****
Constant	2.047	2.086		1.939
	[0.074]**	[0.069]**		[0.022]**
N	26,582	26,582	26,582	26,582

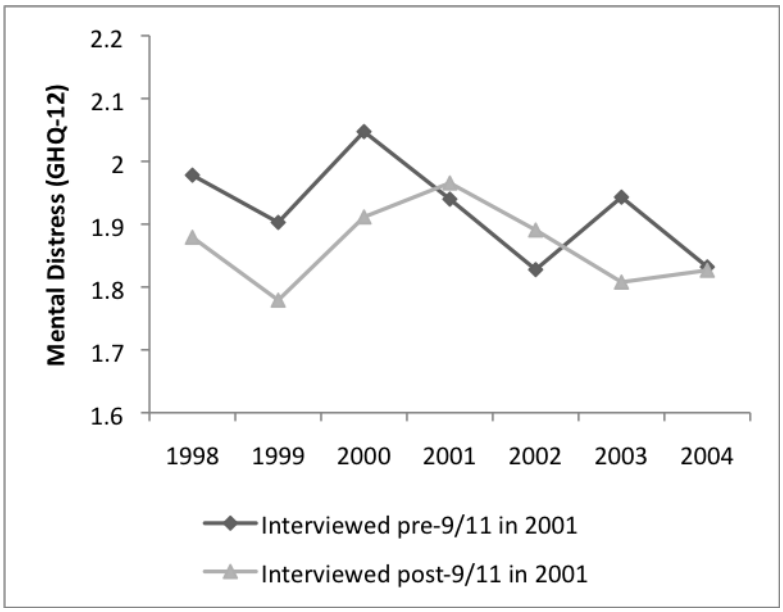
Note: OLS = ordinary least squares; RE = generalised least squares with random effects; OPROBIT-RE = ordered probit with random effects. Mental distress (GHQ-12) takes a value of 0 = no mental distress, 12 = highest level of mental distress. Post-9/11 takes a value of 1 for *both* years (i.e. 2000 and 2001) if the individual was interviewed between 1st January 2001 and 10th September 2001 and 0 between 12th September 2001 and 31st December 2001. Base year = 2000. Standard errors are in parentheses and, in the OLS case, are robust to the clustering by personal identification.
*<5%; **<1%

Table 2: GHQ-12 unpacked regressions – unbalanced sample 2000-2001 (Sample 1)

Dependent variable: Mental distress	Less able to concentrate	Loss of sleep	Less able to play useful role	Less able to make decisions
Post-9/11	-0.026	0.008	-0.035	-0.037
	[0.014]	[0.020]	[0.015]*	[0.013]**
Year=2001	-0.014	0.004	-0.019	-0.040
	[0.015]	[0.019]	[0.016]	[0.014]**
Post-9/11 x Year=2001	**0.008**	**0.004**	**0.021**	**0.043**
	[0.016]	**[0.021]**	**[0.017]**	**[0.015]****
Constant	2.193	1.889	2.059	2.018
	[0.013]**	[0.018]**	[0.014]**	[0.012]**
Dependent variable: Mental distress	**Constantly under strain**	**More problem overcoming difficulties**	**Less able to enjoy day-to-day activities**	**Less ability to face problems**
Post-9/11	0.009	0.028	-0.059	-0.037
	[0.020]	[0.018]	[0.015]**	[0.013]**
Year=2001	-0.010	0.046	-0.043	-0.023
	[0.019]	[0.018]*	[0.016]**	[0.014]
Post-9/11 x Year=2001	**0.005**	**-0.037**	**0.058**	**0.039**
	[0.021]	**[0.020]**	**[0.018]****	**[0.015]****
Constant	2.112	1.804	2.197	2.069
	[0.018]**	[0.017]**	[0.014]**	[0.012]**
Dependent variable: Mental distress	**Unhappy or depressed**	**Losing confidence**	**Feel worthless**	**Less frequency of feeling happy**
Post-9/11	-0.001	-0.041	-0.005	-0.030
	[0.021]	[0.019]*	[0.017]	[0.015]*
Year=2001	-0.022	-0.030	-0.012	-0.033
	[0.020]	[0.018]	[0.016]	[0.016]*
Post-9/11 x Year=2001	**0.027**	**0.034**	**0.009**	**0.044**
	[0.022]	**[0.020]**	**[0.018]**	**[0.017]***
Constant	1.919	1.732	1.444	2.047
	[0.019]**	[0.018]**	[0.016]**	[0.014]**

Note: See Table 1. The responses range from 1. Not at all, ..., to 4. Much more than usual. Hence, a positive coefficient here denotes higher levels of mental distress.

Figure 1: Trends in mental distress before and after the 9/11 attacks – unbalanced panel BHPS 1998-2004 (Sample 2)



Note: This is an unbalanced panel. There were 1,048 individuals interviewed pre-9/11 (7,099 individuals post-9/11) in 1998; 1,449 individuals interviewed pre-9/11 (8,092 individuals post-9/11) in 1999; 1,732 individuals interviewed pre-9/11 (9,263 individuals post-9/11) in 2000; 2,510 individuals interviewed pre-9/11 (13,077 individuals post-9/11) in 2001; 1,779 individuals interviewed pre-9/11 (9,973 individuals post-9/11) in 2002; 1,617 individuals interviewed (10,037 individuals post-9/11) in 2003; 1,467 individuals interviewed pre-9/11 (9,205 individuals post-9/11). Year 2001 is the year of the 9/11 attacks.

Table 3: Mental distress and the 9/11 attacks with multiple time periods and treated groups – unbalanced panel 1998-2004 (Sample 2)

Dependent variable: Mental distress	RE	FE	ML	AR(1)	Collapsed
Post-9/11	-0.138		-0.140	-0.154	-0.150
	-0.074		[0.075]	[0.074]*	[0.064]*
Year=1998	-0.122	-0.078	-0.126	-0.130	
	[0.093]	[0.112]	[0.093]	[0.095]	
Year=1999	-0.159	-0.135	-0.160	-0.178	
	[0.086]	[0.093]	[0.085]	[0.083]*	
Year=2001	-0.143	-0.201	-0.146	-0.155	-0.066
	[0.076]	[0.083]*	[0.076]	[0.073]*	[0.060]
Year=2002	-0.152	-0.214	-0.154	-0.149	
	[0.081]	[0.102]*	[0.081]	[0.082]	
Year=2003	-0.057	-0.112	-0.056	-0.068	
	[0.083]	[0.124]	[0.083]	[0.085]	
Year=2004	-0.146	-0.215	-0.144	-0.159	
	[0.086]	[0.149]	[0.086]	[0.088]	
Post-9/11 x Year=1998	0.080	0.068	0.086	0.089	
	[0.099]	[0.101]	[0.099]	[0.101]	
Post-9/11 x Year=1999	0.015	0.010	0.017	0.035	
	[0.092]	[0.095]	[0.092]	[0.089]	
Post-9/11 x Year=2001	**0.202**	**0.237**	**0.203**	**0.216**	**0.193**
	[0.082]*	**[0.083]****	**[0.082]***	**[0.078]****	**[0.066]****
Post-9/11 x Year=2002	0.154	0.180	0.155	0.154	
	[0.088]	[0.090]*	[0.088]	[0.089]	
Post-9/11 x Year=2003	0.003	0.004	0.003	0.017	
	[0.089]	[0.091]	[0.088]	[0.091]	
Post-9/11 x Year=2004	0.115	0.111	0.112	0.131	
	[0.091]	[0.093]	[0.091]	[0.094]	
Constant	2.626	2.075	2.527	2.572	1.997
	[0.256]**	[1.332]	[0.259]**	[0.259]**	{0.058]**
Control variables	Yes	Yes	Yes	Yes	Yes
N	78,348	78,348	78,348	78,120	26,582

Note: See Table 1. Control variables include month interviewed dummies, gender, age, age-squared, log of real household income, as well as dummies for marital status, labour force status, education, number of children, and regions. Estimates on the control variables are reported in Appendix D. *<5%; **<1%

Table 4: Mental distress and the 9/11 attacks with multiple time periods and treated groups by socio-demographic groups – unbalanced panel 1998-2004 (Sample 2)

Dependent variable: Mental distress	Female	Male	Age<=35	Age>35
Post-9/11	-0.167	-0.102	-0.055	-0.193
	[0.107]	[0.100]	[0.130]	[0.090]*
Year=1998	-0.126	-0.115	0.117	-0.211
	[0.134]	[0.125]	[0.194]	[0.105]*
Year=1999	-0.168	-0.148	-0.073	-0.202
	[0.124]	[0.116]	[0.170]	[0.099]*
Year=2001	-0.249	-0.013	-0.097	-0.174
	[0.110]*	[0.102]	[0.139]	[0.090]
Year=2002	-0.204	-0.082	-0.036	-0.194
	[0.116]	[0.109]	[0.152]	[0.095]*
Year=2003	-0.180	0.100	-0.200	-0.001
	[0.119]	[0.112]	[0.160]	[0.097]
Year=2004	-0.185	-0.090	-0.099	-0.157
	[0.123]	[0.116]	[0.172]	[0.099]
Post-9/11 x Year=1998	0.051	0.109	-0.134	0.156
	[0.144]	[0.133]	[0.202]	[0.114]
Post-9/11 x Year=1999	-0.030	0.070	-0.164	0.113
	[0.133]	[0.124]	[0.180]	[0.107]
Post-9/11 x Year=2001	**0.289**	**0.100**	**0.081**	**0.283**
	[0.118]*	**[0.110]**	**[0.149]**	**[0.097]****
Post-9/11 x Year=2002	0.158	0.144	-0.057	0.259
	[0.127]	[0.118]	[0.164]	[0.103]*
Post-9/11 x Year=2003	0.091	-0.108	0.120	-0.021
	[0.128]	[0.120]	[0.170]	[0.104]
Post-9/11 x Year=2004	0.129	0.094	-0.062	0.196
	[0.131]	[0.123]	[0.181]	[0.106]
Constant	2.939	0.000	1.554	6.033
	[0.375]**	[0.000]	[0.600]**	[0.475]**
Control variables	Yes	Yes	Yes	Yes
N	42,553	35,567	25,078	53,042

Note: See Table 1.

Table 5: Mental distress and the 9/11 attacks – balanced panel (Sample 3)

Dependent variable: Mental distress	**RE**	**FE**
Post-9/11	-0.230	
	[0.124]	
Year=1998	0.030	0.315
	[0.207]	[0.264]
Year=1999	-0.174	-0.044
	[0.145]	[0.166]
Year=2001	-0.548	-0.739
	[0.205]**	[0.225]**
Year=2002	-0.226	-0.444
	[0.138]	[0.209]*
Year=2003	-0.181	-0.490
	[0.141]	[0.275]
Year=2004	-0.169	-0.560
	[0.131]	[0.341]
Post-9/11 x Year=1998	0.096	0.080
	[0.137]	[0.137]
Post-9/11 x Year=1999	0.120	0.115
	[0.137]	[0.137]
Post-9/11 x Year=2001	**0.435**	**0.446**
	[0.137]**	**[0.137]****
Post-9/11 x Year=2002	0.194	0.211
	[0.137]	[0.137]
Post-9/11 x Year=2003	0.045	0.061
	[0.138]	[0.138]
Post-9/11 x Year=2004	0.065	0.098
	[0.138]	[0.138]
Inverse Mill's ratio	0.760	1.140
	[0.646]	[0.676]
Constant	2.386	-2.439
	[1.703]	[3.990]
Control variables	Yes	Yes
N	34,894	34,894

Note: See Table 1. *<5%; **<1%

Table 6: Mental distress and the post-9/11 attacks by month – balanced panel (Sample 3)

Dependent variable: Mental distress	RE	FE
Post-9/11		
12th September-20th September	-0.232	
	[0.141]	
21st September-30th September	-0.228	
	[0.149]	
1st October-31st October	-0.258	
	[0.136]	
1st November-30th November	-0.008	
	[0.207]	
1st December-31st December	-0.112	
	[0.503]	
Year=1998	0.025	0.303
	[0.207]	[0.264]
Year=1999	-0.176	-0.049
	[0.145]	[0.166]
Year=2001	-0.539	-0.722
	[0.206]**	[0.225]**
Year=2002	-0.225	-0.440
	[0.138]	[0.209]*
Year=2003	-0.179	-0.484
	[0.141]	[0.275]
Year=2004	-0.168	-0.555
	[0.130]	[0.341]
12th September-20th September x Year=1998	0.034	0.026
	[0.158]	[0.158]
12th September-20th September x Year=1999	0.037	0.034
	[0.158]	[0.158]
12th September-20th September x Year=2001	**0.327**	**0.331**
	[0.158]*	**[0.157]***
12th September-20th September x Year=2002	0.183	0.186
	[0.158]	[0.158]
12th September-20th September x Year=2003	0.180	0.180
	[0.158]	[0.158]
12th September-20th September x Year=2004	0.181	0.194
	[0.158]	[0.158]
21th September-30th September x Year=1998	0.086	0.064
	[0.165]	[0.165]
21th September-30th September x Year=1999	0.135	0.128
	[0.165]	[0.165]
21th September-30th September x Year=2001	**0.514**	**0.523**
	[0.165]**	**[0.165]****
21th September-30th September x Year=2002	0.195	0.205
	[0.165]	[0.165]
21th September-30th September x Year=2003	-0.072	-0.067
	[0.166]	[0.166]
21th September-30th September x Year=2004	0.065	0.083

	[0.166]	[0.166]
1st October-31st October x Year=1998	0.186	0.174
	[0.150]	[0.150]
1st October-31st October x Year=1999	0.196	0.193
	[0.150]	[0.150]
1st October-31st October x Year=2001	**0.468**	**0.483**
	[0.151]**	**[0.151]****
1st October-31st October x Year=2002	0.200	0.225
	[0.150]	[0.150]
1st October-31st October x Year=2003	0.023	0.049
	[0.150]	[0.151]
1st October-31st October x Year=2004	0.015	0.058
	[0.150]	[0.151]
1st November-30th November x Year=1998	-0.135	-0.152
	[0.229]	[0.229]
1st November-30th November x Year=1999	0.024	0.019
	[0.229]	[0.229]
1st November-30th November x Year=2001	0.488	0.489
	[0.234]*	[0.234]*
1st November-30th November x Year=2002	0.152	0.176
	[0.228]	[0.229]
1st November-30th November x Year=2003	-0.030	-0.009
	[0.229]	[0.230]
1st November-30th November x Year=2004	-0.092	-0.047
	[0.229]	[0.230]
1st December-31st December x Year=1998	-0.057	-0.101
	[0.560]	[0.560]
1st December-31st December x Year=1999	-0.246	-0.267
	[0.571]	[0.571]
1st December-31st December x Year=2001	**-0.659**	**-0.739**
	[0.592]	**[0.595]**
1st December-31st December x Year=2002	0.635	0.582
	[0.578]	[0.581]
1st December-31st December x Year=2003	-0.001	-0.041
	[0.578]	[0.581]
1st December-31st December x Year=2004	-0.391	-0.401
	[0.578]	[0.582]
Constant	2.437	-2.441
	[1.705]	[3.991]
Control variables (including inverse Mill's ratio)	Yes	Yes
N	34,894	34,894

Note: See Table 1. **<1%

Appendix A: Descriptive Statistics

	Sample 1			Sample 2			Sample 3		
	All	Pre-9/11	Post-9/11	All	Pre-9/11	Post-9/11	All	Pre-9/11	Post-9/11
N	26,582	4,242	22,340	78,348	11,602	66,746	34,931	4,612	30,319
Mental distress (GHQ-12)	1.94	1.98	1.94	1.88	1.92	1.87	1.85	1.87	1.84
	(2.99)	(2.99)	(2.99)	(2.99)	(2.99)	(2.99)	(2.99)	(2.99)	(2.99)
Personal characteristics									
Male (%)	45.58	44.46	45.80	45.52	44.43	45.71*	44.91	42.78	45.24*
Age	45.03	47.15	44.63*	46.25	49.03	45.76*	48.09	56.26	46.85*
Ln(household income)	9.41	9.31	9.44*	9.46	9.36	9.47*	9.53	9.35	9.56*
Number of children (age<16)	0.54	0.47	0.56*	0.54	0.46	0.55*	0.56	0.41	0.58*
Self-employed (%)	6.37	5.84	6.48*	6.67	6.19	6.75*	7.06	5.72	7.26*
Unemployed (%)	3.63	4.03	3.56*	3.21	3.38	3.18*	2.05	1.73	2.10*
Retired (%)	20.71	27.69	19.39*	21.50	29.63	20.09*	22.42	43.08	19.28*
Maternity leave (%)	0.33	0.21	0.36	0.42	0.31	0.44	0.49	0.15	0.54
Family care (%)	7.23	6.96	7.29*	7.31	6.92	7.38*	7.28	7.85	7.19*
Student (%)	5.67	6.98	5.42*	4.23	5.12	4.07*	1.78	1.17	1.88*
Disabled (%)	4.44	4.48	4.44	4.36	4.47	4.34	3.45	4.34	3.32*
Government training (%)	0.18	0.14	0.20	0.16	0.10	0.17	0.07	0.04	0.08
Other employment (%)	0.51	0.73	0.47*	0.47	0.54	0.46	0.43	0.72	0.39*
Cohabiting (%)	11.36	8.86	11.84*	11.00	8.41	11.45*	10.32	5.10	11.11*
Widowed (%)	7.39	10.30	6.84*	7.43	10.40	6.91*	7.25	14.87	6.09*
Divorced (%)	5.65	5.45	5.70	5.78	6.00	5.74	5.86	6.33	5.79*
Separated (%)	1.70	1.96	1.65	1.70	1.79	1.69	1.48	0.80	1.59
Single (%)	20.00	21.90	19.65*	18.05	18.96	17.89*	12.96	11.95	13.12*
First degree (%)	9.22	7.61	9.53*	9.82	8.77	10.00*	10.72	7.20	11.26*
Higher degree (%)	2.13	2.19	2.13	2.32	2.36	2.31	2.59	1.65	2.73*

Note: * denotes that the means between Pre-9/11 and Post-9/11 group are statistically different from each other at the 5% level.

Appendix B: Instrumental regression for selection bias

Dependent variable: **Sample 2 into Sample 3**	**Probit**
Cooperation of the respondent	
Good	0.049
	[0.025]
Fair/poor	-0.121
	[0.055]*
Ln(real household income)	0.166
	[0.015]**
Male	-0.046
	[0.023]
Age	0.035
	[0.004]**
Age-squared/100	-0.028
	[0.004]**
Cohabiting	-0.053
	[0.034]
Widowed	-0.072
	[0.046]
Divorced	-0.026
	[0.044]
Separated	-0.147
	[0.061]**
Single	-0.095
	[0.037]**
First degree	0.054
	[0.036]
Higher degree	0.022
	[0.070]
Self-employed	-0.039
	[0.039]
Unemployed	-0.301
	[0.042]**
Retired	-0.043
	[0.039]
Maternity leave	0.155
	[0.073]*
Family care	-0.057
	[0.037]
Student	-0.381
	[0.049]**
Disabled	-0.304
	[0.050]**

Government training	-0.431
	[0.139]**
Other employment	-0.061
	[0.088]
Number of children (aged<16)	0.033
	[0.013]**
Regional dummies	Yes
Year dummies	Yes
N	78,120

Note: Reference groups include cooperation of the respondent: excellent, female, married, education: lower than first degree, and full-time employment. Standard errors are in parentheses. *<5%; **<1%

Appendix C: The coefficients on other covariates (Column 2, Table 3)

Dependent variable: Mental distress	RE
Ln(real household income)	-0.055
	[0.018]**
Male	-0.510
	[0.034]**
Age	-0.002
	[0.005]
Age-squared/100	-0.005
	[0.005]
Cohabiting	0.020
	[0.045]
Widowed	0.553
	[0.064]**
Divorced	0.391
	[0.061]**
Separated	1.299
	[0.084]**
Single	-0.008
	[0.051]
First degree	0.126
	[0.053]*
Higher degree	0.183
	[0.106]
Self-employed	0.069
	[0.052]
Unemployed	1.129
	[0.058]**
Retired	0.296
	[0.052]**
Maternity leave	0.525
	[0.138]**
Family care	0.498
	[0.048]**
Student	0.121
	[0.062]*
Disabled	2.236
	[0.063]**
Government training	0.437
	[0.231]
Other employment	0.656
	[0.136]**
Number of children (aged<16)	0.026

	[0.017]
Interviewed months	
February	-0.045
	[0.086]
March	0.087
	[0.089]
April	-0.015
	[0.097]
May	0.034
	[0.102]
September	-0.074
	[0.067]
October	-0.010
	[0.066]
November	0.134
	[0.068]*
December	0.049
	[0.078]
Constant	2.626
	[0.256]**
Regional dummies	Yes
N	78,120

Note: Reference groups include female, married, education: lower than first degree, full-time employment, and January (note that no interviews took place between June-August). *<5%; **<1%