

# The Microlensing Optical Depth Towards the Large Magellanic Cloud: Is There A Puzzle?

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## ABSTRACT

Using neural networks, Belokurov, Evans & Le Du (2003, 2004) showed that 7 out of the 29 microlensing candidates towards the Large Magellanic Cloud (LMC) of the MACHO collaboration are consistent with blended microlensing and added Gaussian noise. We then estimated the microlensing optical depth to the LMC to be  $0.3 \times 10^{-7} \lesssim \tau \lesssim 0.5 \times 10^{-7}$ , lower than the value  $\tau = 1.2_{-0.3}^{+0.4} \times 10^{-7}$  claimed by the MACHO collaboration (Alcock et al. 2000). There have been independent claims of a low optical depth to the LMC by the EROS collaboration, who have most recently reported  $\tau < 0.36 \times 10^{-6}$  (Tisserand et al. 2006).

Griest & Thomas (2005) have contested our calculations. Unfortunately, their paper contains a number of scientific misrepresentations of our work, which we clarify here. We stand by our application of the neural networks to microlensing searches, and believe it to be a technique of great promise. Rather, the main cause of the disparity between Griest & Thomas (2005) and Belokurov et al. (2004) lies in the very different datasets through which these investigators look for microlensing events. Whilst not everything is understood about the microlensing datasets towards the LMC, the latest downward revisions of the optical depth to  $(1.0 \pm 0.3) \times 10^{-7}$  (Bennett 2005) is within  $\lesssim 2\sigma$  of the theoretical prediction from stellar populations alone.

Efficiency calculations can correct for the effects of false negatives, but they cannot correct for the effects of false positives (variable stars that are mistaken for microlensing). In our opinion, the best strategy in a microlensing experiment is to eschew a decision boundary altogether and so sidestep the vagaries of candidate selection and efficiency calculations. Rather, each lightcurve should be assigned a probability that it is a bona fide microlensing event and the microlensing rate calculated by summing over the probabilities of all such lightcurves.

**Key words:** gravitational lensing – stars: variables: others – dark matter

## 1 INTRODUCTION

The microlensing puzzle is: what is the origin of the microlensing events towards the Large Magellanic Cloud (LMC)? Specifically, what fraction of the microlensing events are caused by known stellar components in the Milky Way and by self-lensing of the LMC, and what fraction by a compact dark matter component in the Milky Way halo. Griest & Thomas (2005) argue that there is evidence for an excess of events above and beyond the contribution of the known stellar components in the Milky Way and LMC and hence there is evidence for compact dark objects in the halo.

There are two microlensing collaborations who have heroically monitored the Magellanic Clouds over many years. They have reported rather different numbers of events. After 8 years of monitoring, the EROS collaboration announced just 3 microlensing candidates towards the

LMC (Lasserre et al. 2000). By contrast, the MACHO collaboration (Alcock et al. 1997) first published an analysis of their 2-year dataset. They found a high microlensing optical depth ( $\tau = 2.9_{-0.9}^{+1.4} \times 10^{-7}$ ) based on an 8 event sample. They suggested that this was consistent with about 50% of the halo within 50 kpc being made of objects with mass  $\sim 0.5M_{\odot}$ . This optical depth value was superseded by the analysis of 5.7 years of data, which indicated a somewhat lower optical depth of  $\tau = 1.2_{-0.3}^{+0.4} \times 10^{-7}$  based on either 13 or 17 events (Alcock et al. 2000).

Belokurov, Evans & Le Du (2004) re-analyzed 22 000 publicly available MACHO lightcurves with neural networks, and provided alternative sets of microlensing events. The subset reanalyzed contained all the microlensing candidates of Alcock et al. (2000), but is only a small fraction of the entire public archive of 9 million MACHO lightcurves. We argued that at least some of the events iden-

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tified as microlensing by Alcock et al. (2000) may in fact be contaminants. We roughly estimated the optical depth as  $0.3 \times 10^{-7} \lesssim \tau \lesssim 0.5 \times 10^{-7}$  (Evans & Belokurov 2004)

Subsequently, the EROS collaboration (Tisserand & Milsztajn 2005) reported an optical depth to the Large Magellanic Cloud of  $\tau = (0.15 \pm 0.12) \times 10^{-7}$  based on 3 candidates found in 6.7 years of data – a remarkably low result. Here, the error is calculated using Han & Gould’s (1995) formula with  $\eta = 2.0$ . Note that the EROS estimate provides an upper bound to the contribution of compact dark halo objects to the total optical depth, as an obvious disk lens event was removed. Very recently, EROS have reported further interesting results based on the clever use of a bright subsample of source stars to minimise contamination (Tisserand et al. 2006). They find only one microlensing candidate in this subsample and suggest that the optical depth due to such lenses is  $\tau < 0.36 \times 10^{-7}$  at the 95 % confidence level. If these low values for the optical depth are accepted, then the stellar populations in the outer Galaxy and the LMC must provide most of the lenses for the known events – as in fact is true in all instances where the location of the lens can be identified. There are two exotic events towards the LMC, for which the location of the event can be more or less inferred. They are the binary caustic crossing event studied by Bennett et al. (1996) and the xallarap event studied by Alcock et al. (2001a). In both cases, the lens preferentially lies in the Magellanic Clouds. In addition, there has been the direct imaging of another of the microlenses by Alcock et al. (2001b), revealing it to be a nearby low-mass star in the disk of the Milky Way.

The claims of Belokurov et al. (2003, 2004) and especially Evans & Belokurov (2004) were challenged by Griest & Thomas (2005). Of course, there is no need to re-enact the epic battle between the mice and the frogs (Homer, 8th century BC) in the pages of this *Journal*. Nonetheless, Griest & Thomas (2005) did make a number of scientifically incorrect statements regarding our neural network computations. The main aim of this paper is simply to set the record straight with regard to the event selection (§2) and the efficiency calculation (§3). In our discussion (§4), we delineate the remaining causes of scientific disagreement and discuss ongoing experiments that may provide a resolution.

## 2 REMARKS ON EVENT SELECTION

Any treatment of this subject should begin with some humbling remarks. Neither Griest & Thomas (2005) with powerful statistical methods nor Belokurov et al. (2004) with neural networks can really claim to have devised methods for microlensing detection that are completely successful. A striking indication of this is provided by the EROS collaboration’s discovery that the event MACHO LMC-23 is a variable star (Tisserand & Milsztajn 2005). The lightcurve for this event is a good fit to a blended microlensing curve. Alcock et al. (2000) report a  $\chi^2$  of 1.452 per degree of freedom. The event was included in their set of confident microlensing events (“set A”). Likewise, Belokurov et al. (2004) assessed the probability of microlensing as  $P = 0.99$ . Therefore, both methods failed.

The implications of this for microlensing surveys are worrisome. There exist classes of variable stars whose

lightcurves are good fits to blended microlensing. They cannot be distinguished from microlensing, except by more accurate photometric measurements or by long-baseline monitoring for repeat variations.

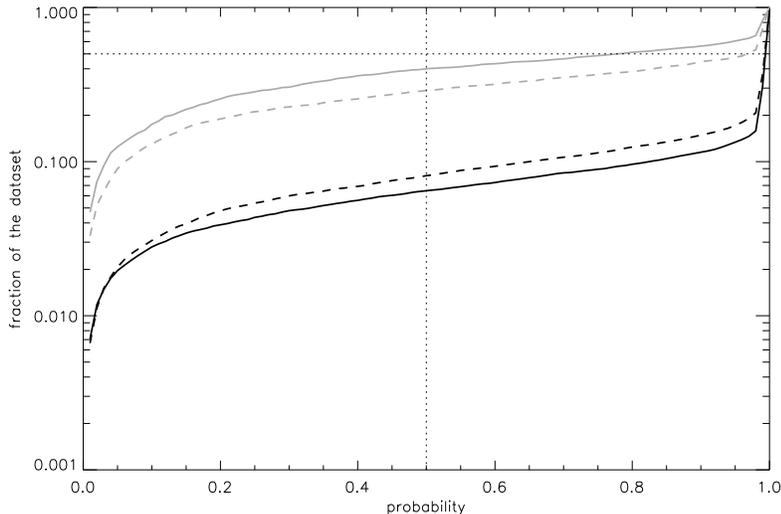
Belokurov et al. (2003, 2004) pioneered the use of neural networks to identify microlensing by single lenses. Our calculations showed that – using the publicly available data – only 7 of the events of Alcock et al. (2000) are consistent with blended microlensing and added Gaussian noise. These calculations are correct, but the noise in the actual experiment is more complicated than Gaussian.

Notice that the selection of events by Alcock et al. (2000) makes the same assumption that the noise is close to Gaussian, in order to proceed with lightcurve fitting and the use of the  $\chi^2$  statistic. However, the data through which Alcock et al. (2000) search for events is not the publicly available data, but is derived from the publicly available data by a cleaning process (see Alcock et al. 1997). We refer to this as the cleaned dataset; it is not publicly available.

Let us compare the noise properties of the public data with the cleaned data.<sup>1</sup> To estimate the amount of variability in the lightcurve, we calculate the  $\chi^2$  value of the constant baseline model. The empirical cumulative probability distributions  $P(\chi < \chi_0)$  are then constructed and shown in Figure 1 as full and dotted curves. The vertical axis is the fraction of the dataset (and so runs from 0 to 1). In a typical cumulative probability distribution, the horizontal axis would be the chi-squared value,  $\chi_0$ . However, here we have converted this to a probability using the fact that, if the errors are normally distributed, the theoretical cumulative probability distribution is known and is the incomplete gamma function  $\Gamma(0.5, \chi_0/2)$  (see Press et al. 1992). In Figure 1, the cumulative probability distributions in the full black and dashed black lines refer to the public data in the MACHO instrumental *R* and *B* filters. The full and dashed grey lines refer to the cleaned data in Johnson *V* and Kron-Cousins *R* (see Alcock et al. 1997). We see immediately that the noise properties of the two datasets are very different. In the ideal case of Gaussian noise and non-varying lightcurves, the probability distributions should have a similar shape but pass through the point (0.5, 0.5). For the public data,  $\sim 90\%$  of the lightcurves correspond to varying objects. By contrast, the noise properties of the cleaned data are much closer to Gaussian.

Let us illustrate the points at issue with an example. Figure 2 shows the *R* and *B* band data of MACHO LMC-4, using the public data. The upper panels show the unbinned data, the lower panels show the binned data. This figure should be compared with the published version of the lightcurve of the same event in Alcock et al. (2000). In particular, the lightcurve of Figure 2 shows secondary activity outside the microlensing bump, for example, at  $t \approx 1400$  days in the *B* band. These datapoints are not present in Alcock et al. (2000) and so they must have been rejected at

<sup>1</sup> One tile only (roughly 3000 lightcurves) of the cleaned data was made available to us for comparison purposes. We thank Andrew Drake for making this possible. The public but uncleaned data are available at <http://www.macho.mcmaster.ca/Data/MachoData.html>.



**Figure 1.** The  $\chi^2$  value of the constant baseline model is computed for each lightcurve using (i) the publicly available data from the MACHO website (black curves) and (ii) the cleaned data (grey curves) used in Alcock et al.’s (2000) analysis. The full and dashed curves are the empirical cumulative probability distributions  $P(\chi < \chi_0)$  in the red and blue passbands respectively. The vertical axis is the fraction of the dataset. The horizontal axis would normally be the chi-squared value  $\chi_0$  in a cumulative probability distribution. However, if the errors are normally distributed, the theoretical cumulative probability distribution is known and is the incomplete gamma function  $P = \Gamma(0.5, \chi_0/2)$ . This is plotted as the horizontal axis. If the noise is Gaussian, then the probability distribution passes through the point (0.5, 0.5), whose location is marked by the dotted lines. As the cleaned data pass close to this point, we conclude that their noise properties are nearly Gaussian. By contrast, the noise properties of the publicly available data are not close to Gaussian at all.

an early stage in the photometric reduction. There may of course have been good reasons to remove these datapoints.

Griest & Thomas (2005) argue that the greater number of lightcurves identified as microlensing by Alcock et al. (2000) is due to the power of lightcurve fitting. This is not the case. Lightcurve fitting will not identify the data in Figure 2 as a microlensing event. To show this, we perform exactly the lightcurve fitting that Griest & Thomas (2005) advocate. The  $\chi^2$  per degree of freedom is 1.73 for the publicly available lightcurve of MACHO LMC-4. This is higher than the value of 1.38 reported by Alcock et al. (2000). In fact, there are no lightcurves in Alcock et al.’s (2000) set A with as high a reduced  $\chi^2$  as 1.73.

In other words, the main differences between the results of Belokurov et al. (2004) and those of Griest & Thomas (2005) are caused by the fact that these investigators search through different datasets. More succinctly, *the publicly available MACHO data are badly polluted with photometric outliers.*

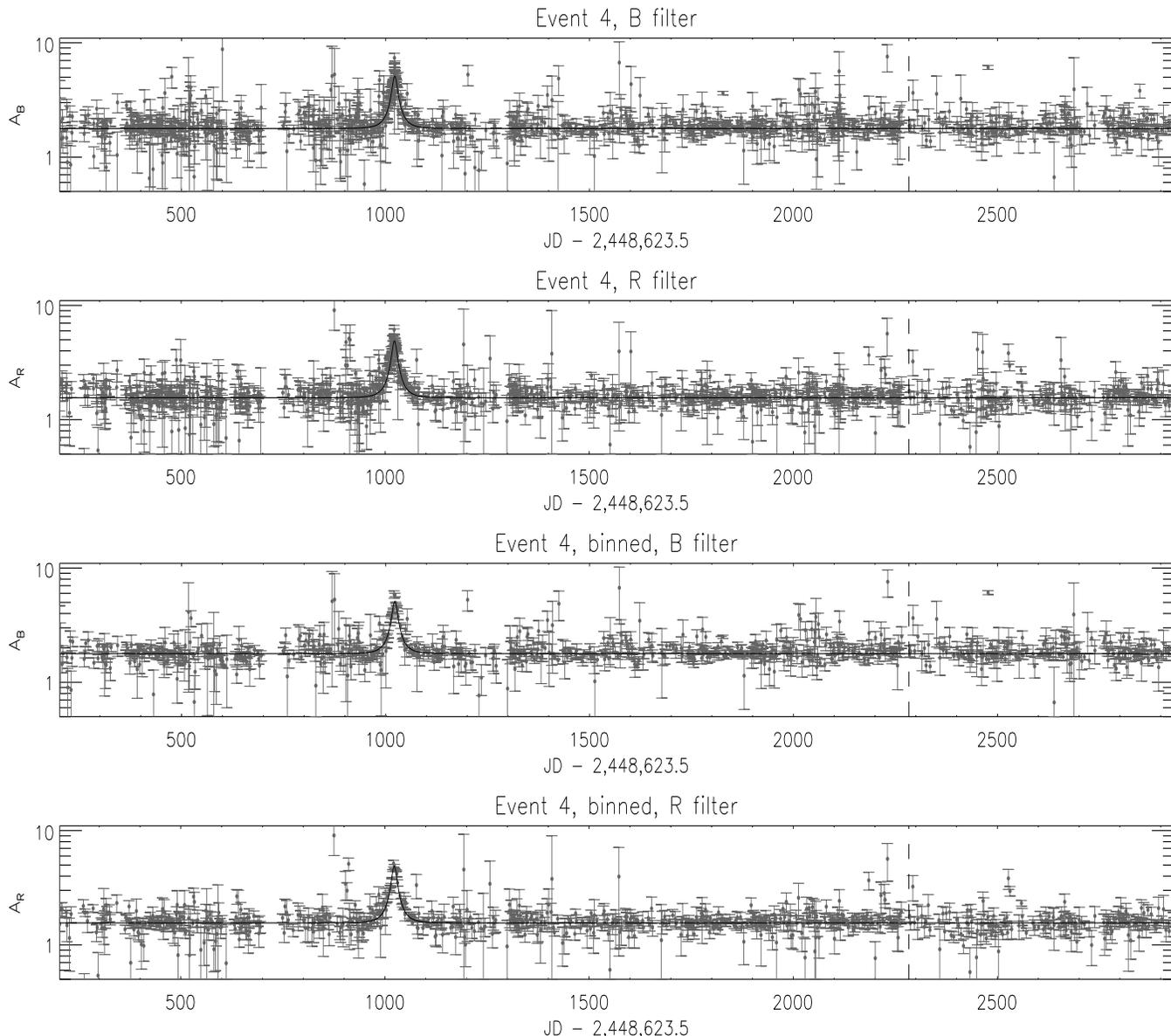
Of course, the MACHO collaboration has much more information on photometric problems than is publicly available. For example, they can remove datapoints based on photometric problems that recur for hundreds of stars in the same field for a troubling exposure (Griest 2005, private communication). At least as judged from Figure 1, they seem to have done a good job. Hence, we believe that the cleaned data are more useful than the public data – it is just a pity that the cleaned data are not generally available.

Belokurov et al. (2004) realised that there are problems with the public data and discarded any data-point that deviates more than  $3\sigma$  from its neighbours. Griest & Thomas (2005) suggest that this procedure removes fast, short duration events and so is partly responsible for the discrepancy.

For each lightcurve, Belokurov et al. (2004) analyse *both* the public data and the data from which  $3\sigma$  outliers have been removed and quote the maximum output from the neural networks (i.e., the one for which the probability of microlensing is the greatest). Hence, if the public data has a very high probability of microlensing, the event is recognized.

Griest & Thomas (2005) assert that Belokurov et al.’s (2004) cross and auto-correlations assume that the photometric data are evenly spaced in time. Neural networks are pattern recognition machines. So, they can be trained to recognize the pattern of a sparsely sampled microlensing lightcurve, without explicit accounting for missing data. Of course, it is a different pattern to a microlensing lightcurve with full time-sampling – but a pattern none the less. All that is needed is to include examples of such patterns with sparse sampling in the training set, as Belokurov et al. (2004) do. Besides, one of the features input to the neural network, namely the Lomb-Scargle periodogram, does not assume equally spaced datapoints (see for example, Press et al. 1992).

Griest & Thomas (2005) have also miscalculated the false positive rate of the algorithm in Belokurov et al. (2004). The false positive rates stated in Belokurov et al. (2004) refer to *the rates at which the common classes of variable stars in the training set are mistakenly identified as microlensing.* They do not refer to the rate at which any lightcurve (whether variable or constant baseline) is mistakenly identified as microlensing. To compute the number of false positives for the whole of the 9 million lightcurves in the MACHO database, we must first calculate how many variable stars are expected? We can estimate this using results from the OGLE-II survey of the LMC (Žebruň et al. 2001). OGLE-II found that 0.8% of all stars towards the LMC are



**Figure 2.** The publicly available  $R$  and  $B$  band data for the event MACHO LMC-4. The upper two panels show the un-binned data, the lower two panels the binned data (with bin-size 1 day). The horizontal axis is time in JD - 2448623.5 (2 Jan 1992), whilst the vertical axis is the linear magnification  $A_{R,B}$ . The peak of the event, which is discernible from the microlensing fit superposed on the lightcurves, is at 1023.0 days and the Einstein crossing time  $t_E$  is 43 days. The corresponding lightcurves using the cleaned data are displayed in Alcock et al. (2000).

variables with amplitude variations exceeding the measurement errors and with periods less than a few years. The MACHO collaboration monitored  $\sim 11.9$  million lightcurves, of which 20% occur in field overlaps (Alcock et al. 2000). So, this implies that the number of true variable stars is  $\sim 85000$ . From the experiment reported in Figure 3 of Belokurov et al. (2004), the false positive rate is 2 in 22000 or  $0.9 \times 10^{-4}$ . Given that the total number of variable stars is  $\sim 85000$ , this means that the false positives amount to less than 8 for the whole dataset of 9 million lightcurves.

However, the experiment reported in Figure 3 of Belokurov et al. (2004) used the public data, not the cleaned data. As we have shown in Figure 1 here, this public dataset

is very noisy and contains over an order of magnitude too many variable objects (mainly caused by artefacts). Hence, when applied to the dataset that the MACHO collaboration actually use, our false positive will certainly diminish, probably by at least an order of magnitude.

Griest & Thomas (2005) point out that the MACHO selection procedure uses around 20 statistical methods, whereas Evans & Belokurov (2004) use only 5 as inputs to the neural networks. They suggest that their suite of statistical methods may be more powerful. In fact, it is difficult to tell unless both methods are run on the same data. However, as Figure 2 of Belokurov et al. (2004) shows, the 5 statistical methods already identify 95% of microlensing events in the

test set. This is an excellent result by any standards – and it is easy to incorporate any further statistics as inputs to the neural networks, if desired.

Finally, Griest & Thomas (2005) mistakenly argue that Evans & Belokurov (2004) only looked for microlensing events among the 22 MACHO candidates and so the efficiency is necessarily reduced. Let us clarify the calculation that was actually done. Evans & Belokurov (2004) examined 22 000 lightcurves, including the 22 MACHO candidates. This number includes the “set A” or stronger microlensing candidates and the “set B” or weaker candidates from Alcock et al. (2000). We made the additional, strong assumption that there are no further microlensing events in the rest of the data. This assumption may be valid or invalid – it is impossible to say unless all the data are made available for testing with neural networks<sup>2</sup>. However, there is some supporting evidence for Evans & Belokurov’s (2004) point of view, as none of the “set B” MACHO candidates passed our neural network selection criterion. Having made this assumption, Evans & Belokurov (2004) proceed – as usual in any efficiency calculation – by calculating the fraction of simulated events that are identified by the algorithm. Such an efficiency calculation does not depend on whether all or part of the original dataset was analyzed.

### 3 REMARKS ON FALSE POSITIVES

Any treatment of this subject should begin with some cautionary remarks on the limitations of efficiency calculations. The optical depth is usually calculated from the data as a sum over events using

$$\tau = \frac{\pi}{4} \sum_i \frac{t_{0,i}}{NT\epsilon(t_{0,i})} \quad (1)$$

where  $N$  is the number of stars monitored,  $T$  is the duration of the experiment and  $\epsilon$  is the efficiency as a function of timescale. There is an important assumption in this formula. The assumption is that the false positive rate is completely negligible. *An efficiency calculation can correct for false negatives (that is, missed microlensing events) but it cannot correct for false positives (that is, variable objects wrongly classified as microlensing).*

By altering the threshold in any selection procedure, we simply generate more false positives at the expense of less false negatives – or vice versa. Even after correcting with the efficiency, different selection procedures will not give the same optical depth, unless the false positive rate is completely negligible for all the thresholds. Alcock et al. (1997) and Alcock et al. (2000) used different cuts and obtained quite different values for the optical depth. This is a broad hint that the false positive rate is not negligible in the Alcock et al. (1997) sample.

Everyone now accepts that MACHO LMC-23 is a variable star (Tisserand & Milsztajn 2005). This false positive was included in the more confident “set A” of Alcock et al.

(2000). “Set B” has a lower threshold and logically must contain still more false positives. In any case, let us emphasise that there is nothing exotic about MACHO LMC-23. It is a variable star that is able to masquerade as a microlensing event because of photometric noise. There is already some indication of this in the comparatively high value of its  $\chi^2$  of 1.452 per degree of freedom.

Given the fact that there are false positives in the Alcock et al. (2000) samples, what is the best methodology for correcting the optical depth? There have been three attempts to do this so far, namely by Griest & Thomas (2005), Bennett (2005) and Evans & Belokurov (2004). All three computations have inadequacies.

Griest & Thomas (2005) attempted to correct for the effects of contamination by removing the contribution of MACHO LMC-23. To see why this is inappropriate, let us consider a model problem in which the cut is only based on a  $\chi^2$  per degree of freedom. Then three further events (candidates 5, 8 and 21) would be discarded, as their  $\chi^2$  per degree of freedom is worse than MACHO LMC-23. This is because the decision boundary between microlensing and non-microlensing cannot have artificially created holes, or any abrupt or sharp features. Of course, the actual algorithm that the MACHO collaboration use is more sophisticated than a cut on  $\chi^2$ . Our point is merely that a misclassified event inside the decision boundary affects its entire neighbourhood. It is not enough simply to remove by hand the contribution of the misclassified event, as Griest & Thomas (2005) do. Their calculation is not a proper accounting of the effects of contamination, even in the optimistic case in which MACHO LMC-23 is the only false positive.

Bennett (2005) tried to correct the optical depth calculations of Alcock et al. (2000) for the effects of contamination by introducing a likelihood estimator. His likelihood estimator is tantamount to assuming that the contamination rate is 1 event out of every 5. Although Bennett (2005) does recompute the efficiencies, this is to take into account a systematic error in the efficiencies used by Alcock et al. (2000). However, Bennett does not take into account the change in the efficiencies caused by the different event selection required to eliminate the contaminants.

Evans & Belokurov (2004) published an estimate of the optical depth allowing for contamination. We argued for more contaminants than either Bennett (2005) or Griest & Thomas (2005) and concluded that the optical depth  $\tau$  satisfied  $0.3 \times 10^{-7} \lesssim \tau \lesssim 0.5 \times 10^{-7}$ , where the range corresponds to the  $\pm 1\sigma$  interval. Evans & Belokurov’s (2004) analysis has two problems. First, as stated earlier, we did not have access to the whole dataset, but to  $\lesssim 1\%$  of it. Second, in our efficiency calculation, we used a simple microlensing lightcurve model with added Gaussian noise and MACHO sampling to test whether events would be found by neural networks. This procedure would be better applied to the cleaned data, which are not available, and so Evans & Belokurov (2004) perforce used the polluted public data.

If a decision boundary between microlensing and non-microlensing is introduced, then it is crucial to know the false positive rate. We have not been able to find such a calculation in the literature for the MACHO experiment. To compute the false positive rate, it is important to use the full gamut of possible variable star lightcurves. In Belokurov et al. (2004), the false positive rate was calculated using

<sup>2</sup> At present, lightcurves can be downloaded on an individual basis (or in small groups) from the MACHO website, thus making downloads of 12 million lightcurves practically impossible. In any case, what is really needed is the capability to make downloads of entire fields of the cleaned data.

standard libraries of variable stars. Another possibility is to apply Feeney et al.’s (2005) adaption of the technique of  $K$  fold cross-validation, which uses the entire dataset itself to provide the range of variable lightcurves.

The alternative to introducing a decision boundary is to assign probabilities to each lightcurve using, for example, the outputs of neural networks. The microlensing rate can be calculated directly from the outputs, without introducing an explicit decision boundary. Every lightcurve makes a weighted contribution to the microlensing rate.

One way of carrying this out is described in Belokurov et al. (2004, see Appendix A). Briefly, the formula

$$\hat{P}(\text{microlensing}) \approx \frac{1}{N} \sum_i y_i \quad (2)$$

is used to estimate the true probability of microlensing. Here,  $i$  runs through all  $N$  lightcurves in the entire data set. The probabilities  $y_i$  are the outputs of neural networks. Ordinarily, the output is the posterior probability of microlensing, given the prior probabilities imposed by the training set. However, Belokurov et al. (2004) showed how to iteratively adjust the outputs so that they converged to the true probabilities given the real-world priors.

The output of this procedure is the probability of microlensing in the experiment monitoring  $N$  stars and lasting for a duration  $T$ . From this, the microlensing rate is

$$\Gamma = \frac{N}{T} \hat{P}(\text{microlensing}) \quad (3)$$

The advantage of this algorithm is that the rate can be computed directly from the dataset, without the intervening steps of candidate selection and efficiency estimation.

This is very different to the approach of all microlensing experiments so far, which have categorized events as either microlensing or non-microlensing. The probabilities assigned are therefore either 1 or 0. Not merely are marginal events incorporated into the optical depth with the same weight as unambiguous events, but – worse still – their contribution is amplified by the efficiency factor as well. The efficiency naturally tends to be low for the marginal events. This may well be part of the reason for the continuing mismatch between theoretical estimates and observational results in microlensing.

## 4 DISCUSSION

Evans & Belokurov’s (2004) suggestion that the optical depth to the LMC may have been over-estimated because of contamination by false positives deserves serious consideration. Indeed, the suggestion receives support from the subsequent results of the EROS collaboration, which was also monitoring the Large Magellanic Cloud. Tisserand & Milsztajn (2005) find the low optical depth  $\tau = (0.15 \pm 0.12) \times 10^{-7}$  based on 6.7 years of data. They also report that MACHO LMC-23 – included in Alcock et al.’s (2000) “set A” of confident events – is actually a variable star. Very recently, Tisserand et al. (2006) exploited the idea of a bright subsample to minimise the effects of contamination and obtained  $\tau < 0.38 \times 10^{-7}$

The main reason for the difference between the results of Belokurov et al. (2004) and those of Griest & Thomas

(2005) lies in the treatment of the photometric outliers. Belokurov et al.’s (2004) calculations use the public data and are valid in the case that the noise is Gaussian. In fact, the noise properties of the public data are non-Gaussian. Griest & Thomas (2005) use the cleaned data – a version of the data in which many photometric outliers have been removed – so that the noise is closer to Gaussian. The cleaned data are not publicly available. The events that Belokurov et al. (2004) claimed as microlensing are reasonably trustworthy. If an event is identified in noisy data, then use of the cleaned data will only improve matters. More problematic are the events for which Belokurov et al. (2004) failed to identify as microlensing, at variance with the original judgement of Alcock et al. (2000). It is impossible to say anything further about these events until either the cleaned data or the algorithm for cleaning the public data are made public. Belokurov et al. (2004) therefore give a final sample of events whose microlensing nature is almost beyond question. This is valuable, as false positives are destructive and cannot be corrected by the efficiency.

Let us also remark that the events Belokurov et al. (2004) claimed as non-microlensing *may* be incorrectly designated (c.f. Bennett, Becker & Tomaney 2005). If so, this is not a fault of the neural network methods, but a consequence of the use of the polluted public data.

Are Griest & Thomas (2005) correct to claim a microlensing puzzle? It is true that the experimental determinations of the optical depth to the LMC are presently uncertain to almost an order of magnitude (from  $\tau = (0.15 \pm 0.12) \times 10^{-7}$  based on EROS data by Tisserand & Milsztajn (2005) to  $(1.0 \pm 0.3) \times 10^{-7}$  based on MACHO data by Bennett (2005)). However, the EROS experiment monitors a wider solid angle of less crowded fields in the LMC than the MACHO experiment. So, blending and contamination by LMC self-lensing are less important for the EROS experiment than for MACHO. The EROS result is therefore an average value of the optical depth over a wide area of the LMC disk, whilst the MACHO value is the optical depth in the central parts. Nonetheless, this cannot be the whole story. The contribution to the optical depth of lensing objects lying in the Milky Way halo varies only weakly across the face of the LMC. So, if the claims that 20% of the dark halo is in the form of compact objects are correct (e.g., Alcock et al. 2000; Griest & Thomas 2005), then this optical depth contribution of this lensing population (approximately  $\tau \sim 0.6 \times 10^{-7}$ ) should be largely independent of position.

The theoretical estimates of the optical depth of the known Galactic components in the direction of the LMC have been computed anew and are listed in Table 1. Using the latest models of the thin and thick disk (e.g., Binney & Evans 2001), we find that their contribution is  $\tau = 0.10 \times 10^{-7}$ . This is a middle-of-the-range value, and both larger (e.g., Alcock et al. 1997; Evans et al 1998) and smaller numbers (Alcock et al. 2000) can be found in the literature. The optical depth of the spheroid is uncontroversial and is  $\tau = 0.02 \times 10^{-7}$ . There is much more dispute about the LMC self-lensing optical depth. Accordingly, we list a number of recent estimates in the Table – our preferred value is  $0.55 \times 10^{-7}$ , corresponding to the zero offset model of Zhao & Evans (2001), which is again a reasonable middle-of-the-range value. Notice from Figure 2 of Zhao & Evans

**Table 1.** The theoretical value of the microlensing optical depth towards the LMC for various known stellar populations in the Milky Way and the Large Magellanic Clouds.

Component	Optical Depth	Notes
Thin and Thick disk	$0.10 \times 10^{-7}$	Eqn.(2) of Binney & Evans (2001) using a local column density of $27M_{\odot} \text{ pc}^{-2}$ and a radial scalelength of 3.0 kpc
Spheroid	$0.02 \times 10^{-7}$	Standard $\rho = 1.18 \times 10^{-4} (r/R_0)^{-3.5} M_{\odot} \text{ pc}^{-3}$ spheroid of Giudice et al. (1994)
LMC disk/bar	$0.55 \times 10^{-7}$	Zero Offset Model of Zhao & Evans (2001)
LMC disk/bar	$1.0 \times 10^{-7}$	Non-Zero Offset Model of Zhao & Evans (2001)
LMC disk/bar	$0.05 - 0.80 \times 10^{-7}$	Models of Gyuk, Dalal & Griest (2000)

(2001) that the LMC optical depth is roughly constant over the central 2 kpc of the LMC bar. Adding these numbers up, the total optical depth contribution to the LMC from known sources is  $0.67 \times 10^{-7}$ . The error on this theoretical estimate is large, as it controlled by the poorly known extension along the line of sight of the LMC.

The most recent experimental determination of the optical depth from a MACHO collaboration member now stands at  $\tau = (1.0 \pm 0.3) \times 10^{-7}$  (Bennett 2005). In our judgement, Bennett’s calculation is an overestimate. Nonetheless, even accepting his value, the experimental optical depth is only just over  $1\sigma$  away from the theoretical estimate from known populations. There is no major puzzle! This is especially the case given the uncertainties in the physical depth of the LMC. For example, Weinberg & Nikolaev (2000) detected a spread of a few kiloparsecs in distance among their 2 Micron All-Sky Survey of LMC stars. If this is a true indication of the line of sight depth, then the LMC optical depth is still higher than we have assumed, and even the remaining small discrepancy melts away.

There are a number of ongoing experiments that may help solve the microlensing puzzle in the near future. First, and most promisingly, the super-MACHO survey (Stubbs 1999, Becker et al. 2004) has the specific goal of identifying the location of the objects producing the microlensing events. The survey has been taking data since 2001 and has extensive coverage of the face of the LMC. If the lensing objects lie in the halo, there is only a weak gradient across the face of the LMC. However, if the objects lie in the LMC, then there is a substantial gradient. The measurement of this gradient requires an order of magnitude more events than those reported by MACHO and EROS, but should be within the grasp of super-MACHO.

Second, there are a number of microlensing experiments towards the Andromeda galaxy, such as the POINT-AGAPE and MEGA experiments (e.g., Paulin-Henriksson et al. 2003; de Jong et al. 2004; Belokurov et al. 2005), that are now reporting results. These experiments are motivated by the suggestion of Crots (1992) that the event rate to sources in the near and far disks in M31 is different. The lines of sight to the far disk as compared to the near disk are longer and pass through more of the M31 dark halo. However, An et al. (2004) showed that the expected microlensing asymmetry between the near and far disk is overwhelmed by the effects of patchy and variable extinction in the M31 disk. Of course, it is considerably harder to provide convincing evidence that a claimed pixel-lensing event in M31 is due to microlensing as compared to resolved microlensing events. Perhaps because of this, the independent calculations of the

optical depth of the M31 halo currently in the literature are contradictory. Calchi Novati et al. (2005) found 6 short duration events and argued that at least 20 per cent of the M31 halo is in the form of dark, compact objects. De Jong et al. (2006) identified 14 events, but concluded that the signal was equally consistent with both self-lensing and with dark, compact halo objects.

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