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NGC 2419 does not challenge MOND, Part 2

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ABSTRACT

I argue that, despite repeated claims of Ibata et al., the globular cluster NGC 2419 does not pose a problem for modified Newtonian dynamics (MOND). I present a new polytropic model with a running polytropic index. This model provides an improved representation of the radial distribution of surface brightness while maintaining a reasonable fit to the velocity dispersion profile. Although it may be argued that the differences with these observations remain large compared to the reported random errors, there are several undetectable systematic effects which render a formal likelihood analysis irrelevant. I comment generally upon these effects and upon the intrinsic limitations of pressure-supported objects as tests of gravity.

Key words: gravitation – globular clusters: individual: NGC 2419 – dark matter.

1 INTRODUCTION

Ibata et al. (2011a, henceforth I1) have claimed that observations of the distant globular cluster NGC 2419 constitute a severe challenge for modified Newtonian dynamics (MOND), and that this object is, in fact, a ‘crucible’ for theories of gravity. This claim was based, primarily, upon Newtonian and MONDian Michie models in which the phase space distribution is assumed to be of specific form, leading to a density cut-off at a finite radius. In my initial response to their paper, I argued that these models were too restricted and presented high n polytropic MOND models (Sanders 2011, henceforth S11), in which the radial velocity dispersion decreases with radius; to the eye in any case, such models appear to be quite consistent with both the observed radial distribution of surface brightness and the radial profile of velocity dispersion in this cluster. Furthermore, I cautioned that it is questionable to rely on formal errors in likelihood analyses of observations which may well contain observational or intrinsic systematic effects.

Ibata et al. (2011b, henceforth I2) responded with their own analysis of polytropes and assert that they had already, in effect, considered, and ruled out, a range of non-isothermal MOND models which fitted the data better than the polytropic model that I presented; that, in any case, Newtonian Michie models fit the observations significantly better than the MOND non-isothermal models. Again, this conclusion is based upon the use of random errors in a likelihood analysis. It is primarily this point that I wish to address here because it has general relevance to the interpretation of astronomical observations. But first, I present a new model in which the polytropic index runs with radius.

2 MODEL AND DISCUSSION

I2 point to the problem for polytropic MOND models as a tension between the predicted surface density distribution and the velocity

dispersion radial profile. This can be summarized, in my own words, as follows: to more precisely fit the fall-off of surface density within the very small formal errors, in particular near the outer cut-off, the polytropic index must be rather large ($n > 15$); that is, the system should be closer to isothermal. But to fit the radial decrease in the line-of-sight velocity dispersion, the index should be smaller ($n \approx 10$). This result is fairly insensitive to the anisotropy radius – the distance beyond which radial orbits begin to dominate.

One obvious solution to this problem is to allow the polytropic index to increase with radius (there is nothing sacred about the rigid polytropic pressure–density relation). Therefore, I have considered several prescriptions in which n increases beyond 100 pc or so, for example,

$$n = n_0 \exp[r/r_p]. \quad (1)$$

Below, I show the results for one such model. Fig. 1 is the observed surface density of stars as a function of radius (points) and that predicted by the model (solid curve), and Fig. 2 is the same for the line-of-sight velocity dispersion. For this model, the central radial velocity dispersion is taken to be 7.7 km s^{-1} , the central density is $38 M_\odot \text{ pc}^{-2}$, the anisotropy radius is 11.5 pc, the central polytropic index is 9.2 and the scale length for the growth of the polytropic index (r_p) is 850 pc. Note that with a variable polytropic index, the structure equation (Jeans equation) for the run of density contains an additional term reflecting the gradient in n . The total mass of this model is $6.04 \times 10^5 M_\odot$, yielding a mass-to-light ratio of 1.4. The value of the MOND acceleration parameter is $a_0 = 10^{-8} \text{ cm s}^{-2}$ and the MOND interpolation function was taken to be standard form as in S11.

Now, is this an acceptable match to the observations? It is clearly an improvement over my initial polytropic MOND model with a fixed n , particularly in matching the surface density distribution while maintaining a reasonable representation of the velocity dispersion profile. But Ibata et al. will probably tell us, based upon a maximum likelihood analysis, that it is completely ruled out by the observations or that it is 138 times less probable than the best

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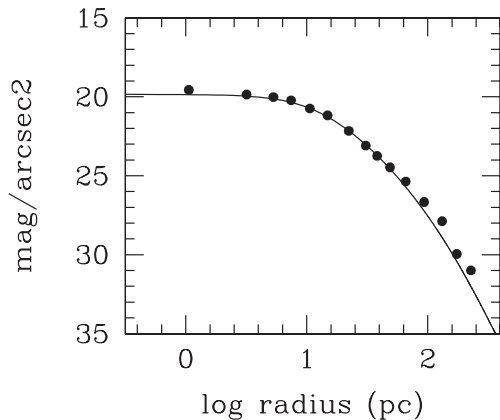


Figure 1. Surface brightness distribution in the globular cluster NGC 2419 (points) compared to the MOND non-isothermal (polytropic with running index), non-isotropic model described in the text.

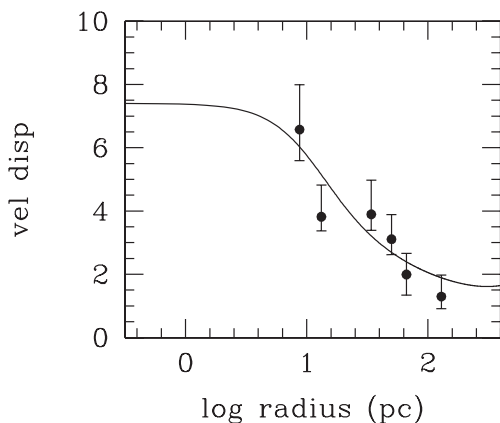


Figure 2. The observed radial dependence of the line-of-sight velocity dispersion in NGC 2419 (points) compared to that of the MOND model shown above.

Newtonian Michie model. Such claims, of course, rest on the fact that the differences between model and observations, while small to the eye, are still larger than the reported measurement errors (comparable to the size of the points in the surface density distribution). But these statements are misleading because they give an impression of precision that is not, and *cannot* be, present in astronomical observations of one such object.

I2 point out that in spherical symmetry, given the anisotropy factor of a model, $\beta(r)$, there is a unique relationship between the run of radial velocity and the density distribution. This is certainly true, but the observed quantities are not the radial velocity or the density but the projected versions of these quantities. We know very well that small errors in the surface density can translate into significant differences in the true density and, hence, in the line-of-sight velocity dispersion (I2 admit that there are problems with de-projection particularly in the outer regions where the errors are larger).

But let us assume that I1 and I2 perfectly understand the systematic and random errors in their observations, and that these are vanishingly small. Then there remains a number of ‘known unknowns’ – or more precisely ‘known unknowables’, for example, the symmetry of the cluster. I1 and I2 argue that the cluster appears to be quite round and that this supports the assumption of spherical symmetry. But if the cluster is oblate or prolate with the symmetry

axis lying near the line of sight, then the cluster would still appear to be round without being spherically symmetric. This would clearly alter the relation between the run of line-of-sight velocity dispersion and the projected density distribution, easily by more than the random measurement errors.

Then there is the unknown of rotation. I1 consider an earlier specific claim of rotation and dismiss this as an effect of small number statistics. But they cannot dismiss the possibility of rotation in general. Rotation is most likely to be present in the outer regions where a systematic velocity of about 4 km s^{-1} would be sufficient to reduce the model velocity dispersion at the outermost measured point in Fig. 2 to the observed value. If the rotation axis were within 30° of the line of sight, its projection would be less than 2.0 km s^{-1} ; given the lower surface density of stars near the cut-off, it is difficult to believe that this would be detectable even in the very careful observations of I1.

The point is that conceivable but undetectable intrinsic systematic effects vitiate the value of likelihood analyses (I have not even mentioned the ‘unknown unknowns’ – effects that we have not thought of). If so, it would not be the first time that sophisticated statistical analyses have been applied to astronomical observations dominated by systematic effects. While Ibata et al. are certainly aware of these various caveats (I1), they continue (I2) to place an unrealistically high value on ‘the objective tool that is statistical inference’.

There are fewer ambiguities with galaxy rotation curves. This is essentially because a two-dimensional object (presumably a highly flattened disc) is being projected on to a two-dimensional sky. There is one component of velocity and the vector most often lies in one plane. The optical appearance of the disc combined with a two-dimensional radial velocity field can generally unambiguously determine the projection, or at least reveal when problems – such as non-circular discs, non-planar motion, warping or non-circular motion – are present. As is well known, MOND works extremely well in predicting the shapes of rotation curves using the observed distribution of baryonic matter (Sanders & McGaugh 2002). But even so, the rotation curves in a few per cent of these well-specified systems are not in agreement with those predicted by MOND, nor would we expect them to be. A perfect theory would not precisely predict the rotation curves of all disc galaxies because of the intrinsic uncertainties.

It is worse for spheroidal pressure-supported objects because of uncertainties due to projection – shape, rotation – as well as the degree and radial dependence of anisotropy of the velocity field and, in relaxed systems such as globular clusters, the possibility of mass segregation and varying mass-to-light ratio. If Ibata et al. had several such objects, all showing the same discrepancies, their case would be stronger, but with only one cluster, the small differences between model predictions and observations are hardly definitive. I point out that opposite conclusion has been reached by Scarpa et al. (2011) based upon observations of distant globular clusters which appear to be inconsistent with Newton (see also the more general discussion of Baumgardt, Grebel & Kroupa 2005). The point is that strong statements on the nature of gravity cannot be made from observations of a single such cluster given the uncertainties intrinsic in astronomical observations of a three-dimensional object projected on to two dimensions.

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