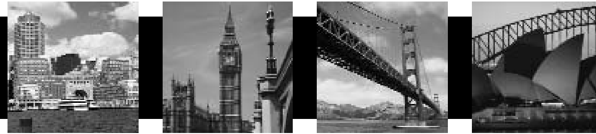
GMO

Quarterly Letter October 2003



Jeremy Grantham, Chairman

The Greatest Sucker Rally in History?

The Third Quarter and 'Bear Market Rallies'

The third quarter was right back into classic 'bear market rally' mode: a huge rally in last year's crushed stocks, and leadership by growth, low quality, and small cap. This of course was not GMO's type of market, unlike the second quarter where we had an unexpected broad value rally, which I reluctantly conceded felt like a serious bull market.

Even small cap, usually at least a modest GMO bias in recent years, was not extraordinarily helpful as we had scored small cap as fully valued relative to large by the beginning of the year, and only our deliberate inertia (a slow 18-month cycle of slicing out of old investments) left us modestly overweight small in the U.S. and substantially overweight in foreign quant, although less than half of our peak bet.

I concede that bear market rallies are a fairly nebulous concept because you cannot be sure what they were until later – the only proof of a bear market rally is that you go to a new low in the not too distant future. But despite this reservation, I cannot resist noodling with the concept.

The characteristics usually attached to a bear market rally are:

a. the prior low was not particularly cheap;

b. the leadership reverts back to that of the prior bull market;

c. the rally is sharp, unusually persistent while it lasts, and has a speculative tone, perhaps because investors are trying to make up lost ground;

d. investors' hearts were only half broken by the previous low in the market, allowing confidence and speculation to recover rapidly.

How do the prior three great bubbles and busts score on this front? (They are: U.S. from 1929 to 1932; U.S. from 1965 to 1974 (or 1982); and Japan from 1990 to 2002 (?).

a. **Value**

All three were scrap iron value or barely half long-term trend. *FAIL*

b. **Leadership**

The changes in the market leadership in Japan and the U.S. in 1932 and 1974 were dramatic, as everything changed; in the U.S. in 1982 they were merely very substantial as energy and commodities declined and value and all small cap rallied. *FAIL*

c. **Sharp, speculative rally**

After 1982 there was some speculation otherwise, *FAIL*

d. **Heart-broken**

All three pass the test and stock ownership halved. *VERY FAIL*

How does this current rally in the aftermath of the fourth great bubble stack up?

a. **Value**

September 2002 low was barely lower than the prior two great bubble highs (19 times to their 21 times). *Spectacular PASS*

b. **Leadership**

Classic reversal to prior bull market leadership, especially growth and tech. *Spectacular PASS.*

c. **Sharp, speculative rally**

This rally has shown substantial outperformance of low quality and rapid build-up of speculation in which Nasdaq margin debt has risen to new highs! *Handsome PASS*

d. **Heart-broken**

Investor confidence has quickly bounced into top quartile levels and newsletter confidence has rebounded to 1999 levels! Cash holdings in funds are also way below normal! *PASS*

The minor rallies in 2001 to 2003 also pass the tests, not surprisingly. Each time we had very strong rallies in the heroes of the prior bubble – tech, growth, and the real internet flakes of the prior cycle. Pumatech remember, doubled three separate times last year and was still down for the year! Well, as Crocodile Dundee would say, "You call that a rally?" Since its low last October, Pumatech climbed 40 times. Yes, 40 times! From 18 cents (down from $100) to $7.21. Pumatech, for the record, has been my selection of the quintessential stock 'flake' for 4 years. This is nothing personal. The company may be a fine, tiny company on its way to break-even, but the stock is flakey. Accompanying Pumatech this year were a substantial percentage of the survivors of the tech IPO frenzy of the late 90s, led in size by Amazon (+211% YTD) and Ebay (+58% YTD). As in 1998 to 1999, growth beat value and tech trounced everything.

But, you may answer, this bear market rally is **bigger** in some ways (the Nasdaq is up over 50%, for example) than any previous bear market rally and certainly **longer**: no other bear market rally after the three great bubbles broke in 1929, 1965, and Japan in 1980 came close to this performance. And this is true! But it is also true that more stimulus and moral hazard has been offered to this rally than any previous one, by a wide margin. It is reasonable, therefore, to expect a big response and we are certainly getting it.

But Ben Inker, more cold blooded than I and less interested in semantics says, "Who cares what you call it, it's going to end badly eventually because it's overpriced."

Third Quarter Performance

Both the quarter and the year so far have had mixed performance for GMO funds, certainly by the standards of the last 3 years, but with two mitigating circumstances. First, all the equity markets are up a lot, and, in the average up month, all our funds tend to lose a little to the benchmark (happily more than offset by downside outperformance in the past). Second, in the U.S., growth has beaten value, 'low quality' has beaten 'high quality', and momentum has failed as last year's wiped out stocks have come surging back. Value, high quality, and positive momentum simply work better with our methodology. *The Financial Times* a month ago had a tidbit that characterized our U.S. problems: the 25% of stocks that were down the worst last year were up 44% this year; the almost 25% of the companies that had no earnings at all were up 41%, but the cheapest 25% on p/e were up only 9.4%! Given this, the underperformance of some of our U.S. funds seems reasonable. In international and emerging, value continued to work through most of the quarter, although the U.S. low quality infection spread rapidly. By the end, I was relieved to see two of our funds that are important to asset allocation – international small cap and emerging – have a good third quarter. Asset allocation, with its huge bear market bias historically, continued to walk on water with a 5.7% YTD gain on its benchmark as I write (October 20), and ahead of the S&P 500's +20% despite holding 35% fixed income. (But I apologized for our good fortune last quarter so I won't again.)

The Presidential Cycle and the Greatest Sucker Rally in History

This topic will be addressed at our Fall Conference and will be written up as a separate piece and sent next quarter. So please bear with me, as many clients do not come to the conference. The argument very briefly is:

- The third year of the Presidential Cycle is used by the administration to attempt to stimulate the economy for year four to create a favorable re-election environment.
- This President, perhaps learning from his father's error, succeeded in having truly record stimulus.
- This stimulus has a moderate effect on the economy in years three and four, but has an extravagant effect on the stock market.

This is partly the traditional effect of lower rates, but is largely psychological; consumer confidence rises, which is coincident with higher p/e's, and we hypothesize that investors generally feel some substantial underwriting of their risks, or moral hazard, by the Fed and the Administration, who imply that they will keep money available and rates low for a chunk of time so that investors can speculate at low risk.

The net effect on the U.S. market is remarkable: since 1932, years one and two have been 4.5 points below average, **year three 8 points over** average, and year four, 1 point over.

Also remarkable is that this U.S. Presidential Cycle effect has been stronger in the UK than in the U.S. since 1932!

(Gallingly, there is absolutely no Prime Ministerial Effect for the Brits!) Since 1970, when good data starts, the effect of the U.S. Presidential Cycle has even been one third as strong in continental Europe and two thirds as strong in Japan. Yes, Japan! Where everything is always considered independent.

Notably, in year three, other normally important influences seem to be swamped by this Presidential Effect and either disappear or are muted. For example: the value of the market (in price/earnings or price/book terms), which is usually moderately indicative of next year's performance, appears to have no effect. In round numbers, all third years are up and 1999, by far the most expensive year ever recorded then, kept going straight up like a good third year. Similarly, the substantially powerful January effect (the strong tendency for January performance to predict the balance of the year) also bounces off year three: witness this year with its slightly down January.

Interestingly, the sector effects in year three are completely compatible with increased confidence and an increased willingness to speculate under the protective umbrella of the Administration and the Fed. Growth, small cap, and low quality all do well in year three just as they are doing this year. Growth stocks beat their average performance relative to value by 5% in year three, small beat large by 6.5% over normal, and low quality beat high quality by 2% over normal.

Outlook for 2004

Yet year four, in complete contrast to year three, is a reasonably normal year. The fourth year outperforms by a statistically insignificant 1% over normal, and small cap is also near normal. What is interesting and surprising to us, however, is that low quality has a poor year and value has its best year. So, if 2004 is an up year, we may do better (or at least less badly) than we would have expected otherwise.

The exceptional fiscal and monetary stimulus program appears to have worked quite well this time, and we expect a continued decent economic recovery and quite good profits for a while longer into next year. These conditions would typically cause a rising market and a growth and speculative tone at least until next year.

Next year, though, anything can happen. The stimulus program will still be having a beneficial lagged effect, **but** the 50%+ odds for a continued rally next year, that I gave in a previous quarterly letter, begin to look vulnerable for several reasons. First, our new research shows, to my surprise, that the Presidential Cycle is largely played out by year four. Second, the fourth year's performance is normally sensitive to the market's aggregate value (unlike year three), and the current market has been rising in an accelerating fashion, which is characteristic of mini (or maxi) bubbles, but more critically has already carried the market to 24x trailing normalized earnings and at this rate, would be in the 25x to 30x range by year end!

So it would be prudent to revise the odds of a continued rally next year to below 50%, though given the exceptional stimulus, the odds of a continued rally should still be reasonable, say 40%. If this year's rally continues at current rates, the odds should fall considerably further from there. So this is a significant and relatively rapid change in my view on next year. I had really hoped for a very slow market advance deep into next year during which we would very slowly and reluctantly increase our defensiveness until later in the year, when we would batten down the hatches and try to be heroes in any ensuing decline.

Outlook for 2005 and 2006

The outlook for 2005 and 2006 unfortunately still looks like a black hole however one massages the data for next year. That would be a very likely time to take this market down to fair value (16x) or below. For as mentioned in earlier quarterly letters, in the first two Presidential Cycle years all the house cleaning – like moving against excessive debt – gets to be done, and debt levels are the highest ever and still growing. This time the chances that small cap or value stocks will materially buck the trend seem slim or none, since they are both fully valued against the market, although in a **major** decline value should help a little. Foreign developed also seems very vulnerable to a sympathetic decline if the U.S. market falls, although given the record **relative** cheapness of foreign developed, the decline should be substantially less and probably further helped by a continued weak dollar. Emerging market equities, despite their huge move, is now the only cheap equity subset, and only slightly cheap at that. But its economics look stronger than in developed countries and there is enormous institutional interest. We have said for years that the reasons to own emerging are that it's different, and that in one 12-month period someday it would double. If the U.S. market hangs in next year, we are probably in

that year. If the U.S. falls (and a faltering economy would still be the most likely catalyst) then emerging might still hang in or at least fight a tough rearguard action.

Summary

Today we have substantially the worst prospects for long-term global investment returns of my 35-year career when all asset classes are considered, particularly for U.S. centric investors. The asset classes **collectively** are simply the most overpriced they have been. There are no large categories that are good hiding places, unlike March 2000, which offered real estate, REITs, all bonds (especially TIPS), small cap value everywhere, and emerging country equities. Only the huge, politically driven stimulus gives cause for hope, and that is for a short-term reprieve or rather a 'stay of execution'. In the longer run, boring old value is **extremely** predictive and at 24x trailing earnings, the 7-year forecast is below -1% a year real return for the S&P 500, to be followed after our assumed 7-year decline by a normal 5.7% a year return. Of course, if the S&P reaches its normal p/e of 16x faster than 7 years, then the short-term pain will be commensurately greater.

Welcome to the unexpectedly large number of new clients; I hope my comments are not too shockingly gloomy. The good news is that my letters are not usually this long, at least not when the attachment is included.

The attachment is an opening salvo on the effect on performance of asset size and how GMO is trying to cope with it.

GMO

Special Topic October 2003

Jeremy Grantham, Chairman

The Size of Assets and its Effect on Performance

Two of our best performing strategies closed to new accounts on September 30 – Emerging Market Equity and Emerging Country Debt – both candidates since their inception for the best performance in their respective categories. Size of assets in any style is the ultimate barrier to adding value, and is the perfect example of the Peter Principle: do well with 2 billion and they'll give you 4 and keep on giving until your good performance has gone.

The appeal of extra marginal business in any business is enormous because some costs are fixed, but in the investment management business, the 'cost of goods' can be small and there can be a strong illusion that there is no material marginal cost at all so that an extra dollar of revenue becomes a dollar of profit. Because of the extreme profitability of the next dollar of revenue, it is desperately hard for a very commercial enterprise to refuse it, and a public company can argue that unlimited growth is justified by its fiduciary responsibility to its stockholders to maximize the firm's profits. In any case, they overwhelmingly act as if this is indeed a guiding principle and few funds are closed. The exception of course is the hedge fund business, and this is interesting for it reflects its different incentives. Hedge fund managers' incentives are not perfect from a client's perspective; they are not paid to maximize the client's performance, but, second best, they are paid to maximize the total dollar outperformance and to do so with absolute performance that at least compares well with competitors. (For example, they are likely to prefer producing 20% performance with $300 million over 25% performance with $100 million.) Institutional long only managers, in complete contrast, are paid to maximize their assets under management, so it should not be a major surprise that this is apparently what they try to do.

Fifteen years ago or so I proposed at one of our client conferences a rule for relating size of assets to value added, or alpha: every time you double your assets, you lower a positive alpha by 30%, or if you quadruple your assets, you will halve your alpha if you prefer. Fifteen years later, it still seems like as well informed a guess as I can come up with.

There has been a considerable amount of nonsense written on this topic. I believe that **every** professional investor knows that it is an ironclad law that size reduces outperformance, but I also understand the investment guild's vested financial interest in muddying the water.

The basic truth is that as you add assets, you have three disagreeable alternatives. You can either add more stocks or buy more of the original list, or both. As you extend your list, you dilute the one or two **brilliant** stock ideas that many good professionals have every now and then, and you dilute the dozen or so **good** ideas. You are quickly into your "B-team" stocks, and eventually you are forced to buy anything that is merely acceptable. If the 'merely acceptable' beat or even equal your highest confidence bets, then you have a very eccentric talent.

It is even easier to understand the point that buying more of the same idea increases the true transaction costs, which eats into your outperformance. Instead of buying 10% of the daily volume for 5 trading days to complete a position, you are in there for 20 days or, finally, months on end buying every day. Alternatively, you can pull back for a while to let the stock cool down, but with a strong alpha, time is money and as you wait, other people get the same good idea. With more money, you are not only pushing the stock more yourself, but allowing more time for others to push with you and share the benefits. I suppose there is yet another alternative and that is to buy more of the daily volume. This is severely bounded at the top as it's hard to buy over 100% of a day's volume, but even at 40% to 50% you are fairly obviously courting disaster.

This problem is not confined to individual stocks, but applies also to larger ideas. In international investing, for example, if a central idea is that Austria is cheaper than other countries, the relative illiquidity of that whole

market will impose severe size or cost limitations; buy less of your best idea or pay more.

There are few unarguable first principles in investing, but I believe larger size equals smaller outperformance to be one. So why have the academics, free of the commercial vested interest, not proven it? Because it's very difficult to prove without a long-term controlled experiment. The time that has been wasted comparing large mutual funds with small ones is impressive. Large funds, of course, get to be large primarily because they are good, and many small funds stay small because they are not. How can one prove that a firm with, say $50 billion in emerging market equities (there is no such firm) would have done even better than they did had they had one tenth of the money. It cannot be done.

Perhaps the best try would be to take the largest 10 funds in, say 1960 and see how they did for 5 or 10 years against funds sized 90 to 100 in 1960. And repeat every 5 years. It doesn't feel like scientific proof, but it might be indicative. When we have time, we will try it and keep you informed, but since we completely believe this whole issue to be self-evident on first principles, it is not at the top of our agenda.

The best counter argument is that by adding more and more good people, you can pick more and more good stocks. The trouble with this is that, like diamond or gold mining, there are only a few great strikes to be had. In a platonic sense, if everything were known to a person of ultimate wisdom, there are at most 50 truly underpriced stocks in the largest 1000. Two or three good old pros might get 25 of these and ten more pros might get 20 more, leaving the last five good ideas to the next 50 pros you might hire or, indeed, the next 500 old pros! The Law of Diminishing Returns exists in almost everything, and in few areas more than investment management.

So for now, let us assume that the point is proven – size hurts. What is GMO to do about it? For at least 25 years I have had some apparently contradictory beliefs. First, I believed it was an exciting challenge to help build a large and profitable firm. Second, I believed the main characteristic of a good money manager was reasonably steady outperformance. Third, I knew that my partners and I wanted above all to be seen as good money managers, for to repeat my own axiom, "There is nothing more supremely useless than a mediocre money manager." But fourth, my partners and I shared the belief that size impacts performance. The way to reconcile or compromise with the conflicts was to have a very broad product line, doing everything we thought we could do that did not compete with our other products, and to close each product down at an appropriate size.

GMO's history at least suggests that our heart is in the right place:

1. Dick Mayo and I, along with Chris Darnell, closed our first product, U.S. Active, at $250 million in 1981 and closed it about as 'hard' as could be done, taking no money from anyone. (There were plenty of temptations, for our first 9 years we were ahead of the S&P 500 by an average of 8% a year.) Eventually we did take a few clients, but only to partially replace those who had left.

2. Foreign active closed to new accounts after its first 3 years at $550 million. (Even though its hit rate for new business presentations was running over 90%. Honest!) After a few years, we decided that the great increase in global liquidity meant that we could manage more money, and we entered an unusual phase of moderate growth, limiting our growth to a **maximum** of 10%. Today's $8 billion seems a reasonable and manageable number given the past performance and the size of the market, and carefully limited growth remains the policy.

3. The closing of the two emerging products (equities and debt) continues this tradition and introduces the topic of the best way to close down or limit growth. There are two relatively different obstructions to steady outperformance. First, there is the steady maintenance of a more or less fixed book of business, and second, there is the incremental impact of new inflow, which all has to be invested fairly quickly lest the manager be debited for poor relative performance if the market rises. These two distinctly different factors suggest at least a two-stage closing. First, at an asset level substantially below the estimated total that the manager feels he can handle well, the manger must limit the inflow of new money, expressed either as dollars or as a percentage increase in assets. This controls the impact of new money to a small fraction of the maintenance impact. Second, as assets carefully grow, the manager can better estimate the level at which **no** new assets should be accepted.

At GMO, we have tried several approaches, but not until now have we had to face an asset class becoming as hot in a few months as emerging country equity has become this year. Last year we set a time target for closing to new

accounts in our Emerging Markets Fund – September 30th of this year. At that time, there was very little money flowing into the asset class and we wanted to give clients as long a lead time to invest as we could. In the interim, as emerging heated up, we ended up with more assets than intended, raising the question of what to do. Our current proposal is to gain experience with the size impact of this new larger amount without any inflow. If with experience, the manager feels the size has pushed us past a desired level of long-term outperformance (this is not necessarily about 1 or even 2 years, which are always buffeted by many other factors than size) then we will **at least** not replace any business that leaves. **At most**, we might reluctantly decide to give money back, a battle plan that should always be considered in any product if market conditions change, say by liquidity drying up.

There are so many factors to be considered in size limitation that we might as well admit it is nearly impossible to be simon pure. No doubt from time to time we will take more money in a given product than we should. But we can and do undertake to go after "the spirit of the exercise". We have designed many of our products quantitatively to handle considerable assets, but we will in **every** product be conscious of the size effect, and we are prepared to close every product at an appropriate size.

We would like GMO, in fact, to be the first broad-based firm who both announces this intention years in advance and lives up to it. Because we have 55 products and still a few more to add, we expect to be able eventually to handle $100 billion or so in today's market terms and still do a good job. It should also be mentioned here that over 90% of our products are quantitative, which is a substantial advantage in building extra liquidity into each fund. We expect (or at least hope) that given favorable circumstances, most of our products will be closed in 10 years. Indeed, I look forward to tottering into work one day when all our products are closed to new clients.

We believe the industry worries far too little about the consequences of unrestricted growth, and our side of the business listens too much to the Goldman Sachs argument that any manager with less than $150 billion is a piker and likely to be squeezed out or absorbed by larger competitors with greater economies of scale. There are two economies in our business. There is a substantial economy of scale in marketing and brand building, and there is a great diseconomy of scale in investment management. I for one will be delighted to hammer at this issue and with any luck seriously embarrass some of our competitors. Given our policy, this issue is gloriously self-serving, but all the best issues are, and this one at least has the virtue of being undeniably correct in general principle.

One question this topic is bound to bring up is which GMO products will be next to close **if** assets continue to flow in. All our hedge funds are very sensitive to size and one big year could close any of them. In long only investing, GMO International Small Cap is a likely candidate. (This is our quant version. The active version – Foreign Small – is already closed.) Similarly, we are already thinking about managing future growth in assets in our flagship Australian equity strategy. Having learned some lessons from emerging markets, we are very likely to close in two stages, and I would expect that we would announce before the end of next year a limited **growth** phase starting when our assets, currently $1.5 billion, hit a maximum $2.5 billion (at today's market level), and probably less. A more complete close would come at a later date when we have more experience.

This topic, we admit, is full of compromises and GMO's main compromise concerns asset allocation. All of our otherwise 'closed' products will be available for broad asset allocation products for some considerable time, including our Multi-Strategy Hedge Fund. We believe, not surprisingly, that the broader funds are the highest and best use of GMO's competence, our best diversification, and importantly, they are contrarian fund buyers: they have been big buyers of GMO funds when the asset class is out of favor – a debt crisis for emerging debt, for example – when client money is leaving. As asset allocation at GMO grows, this will increasingly help stabilize the funds.

Where component funds are otherwise 'closed' to the extent that they do take asset allocation money, they will not otherwise replace departing clients. Typically, it should also be mentioned, the funds that close earlier will be in the illiquid markets that are usually a small percentage of the allocation funds. Where this is not always the case, notably in the absolute return funds, then these allocation funds will have to be the first allocation funds to close.

We would really welcome client feedback on this issue. (I think.)