State Productivity Growth in Agriculture:

Catching-Up and the Business Cycle

V. Eldon Ball
Economic Research Service
U.S. Department of Agriculture
and
Institute of Economics
University Carlos III of Madrid

Carlos San Juan
Department of Economics
University Carlos III of Madrid

Camilo Ulloa
BBVA Research

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ABSTRACT: This paper examines the relation between the business cycle and convergence in levels of agricultural productivity across the forty-eight contiguous states. First, we find evidence of convergence in TFP levels across the different phases of the business cycle, but the speed of convergence was greater during periods of contraction in economic activity than during periods of expansion. Second, we find that technology embodied in capital was an important source of productivity growth in agriculture. As with the rate of catch-up, the embodiment effect was much stronger during low economic activity phases of the business cycle.

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1. Introduction

Two recent studies of the agricultural sector provide evidence of convergence of total factor productivity (TFP) across the U.S. states. McCunn and Huffman (2000) found evidence of 'catching-up' in levels of TFP (*i.e.*, β-convergence), although they rejected the hypothesis of declining cross-sectional dispersion (*i.e.*, σ-convergence).[1] Ball, Hallahan, and Nehring (2004) also found evidence of convergence in levels of productivity after controlling for differences in relative factor intensities (*i.e.*, embodiment).[2] The speed of convergence and whether it is transitory or permanent in nature play an important role in characterizing regional disparities in income (see Abramovitz, 1986; Baumol, 1986; Baumol and Wolff, 1988; and Dowrick and Nguyen, 1989) and, hence, have important implications for the design of policy.[3]

The literature on the empirics of growth provides the tools for investigating the convergence hypothesis. According to this literature (see Barro and Sala-i-Martin, 1992), there is β-convergence if states with lower levels of productivity tend to grow faster than the technology leaders, and σ-convergence if the dispersion of their relative TFP levels tends to decrease over time. Quah (1993a, b) points out that β-convergence is a necessary but not a sufficient condition for σ-convergence. It follows that income inequality across states or regions may persist due to random shocks (*e.g.*, cyclical fluctuations in economic activity) that tend to increase dispersion.

We contribute to this literature by exploring the relationship between the business cycle and convergence. Two alternative explanations have been proposed as to why convergence patterns may be related to the business cycle. The first is based on the pro-cyclical nature of the innovation process (Basu and Fernald, 2001; Geroski and Walters, 1995) and the time lags between technological innovations and the diffusion of technical information (Jovanovic and MacDonald, 1994). According to this argument, productivity leaders tend to innovate more during periods of expansion in response to positive demand shocks. However, due to the existence of informational barriers, productivity followers, who tend to learn by imitation, postpone the adoption of innovations made by the technology leaders until economic downturns. The second explanation is based on the relation between competition and productivity (Escribano and Stucchi, 2008). Productivity followers have more incentive to reduce their costs during downturns when negative demand shocks increase the probability that these firms will exit the industry.

These explanations point to faster rates of convergence during contractions in economic activity and to slower rates of convergence, or even divergence, during periods of expansion. But there are few empirical studies that estimate the impact of the business cycle on convergence. Most either ignore the effects of the business cycle or adjust the productivity measures to eliminate cyclical fluctuations. They do so by either controlling for capacity utilization (Wolff, 1991; Dollar and Wolff, 1994) or by using standard smoothing procedures (Di Liberto, Mura, and Pigliaru, 2008).

An exception is provided by Escribano and Stucchi (2008). Using firm-level data for the Spanish manufacturing sector, the authors test the convergence hypothesis across different phases of the business cycle. They find strong evidence in support of the innovation-imitation hypothesis. Relative TFP levels tend to diverge during periods of expansion and to converge during recessions, a result of both the pro-cyclical nature of the innovation process and the time lags in the diffusion of technical information.

We follow closely the approach of Escribano and Stucchi (2008). First, we test the catch-up hypothesis using a model that ignores the business cycle. Then we investigate the possible impacts of the business cycle on the convergence process by showing how the speed of convergence changes across different phases of the business cycle.

However, we depart from the above-cited study in important ways. First, our focus is on the agricultural sector. This is an important departure since the impact of the business cycle on convergence may differ across sectors of the economy. If prices in the agricultural sector are more flexible than in, say manufacturing, then the impact of the business cycle may be greater in agriculture due to 'overshooting' of prices (see Rucker and Sumner, 1997).[4] On the other hand, recent empirical evidence suggests that convergence may be faster in agriculture, the result of more rapid dissemination of technical information (Martin and Mitra, 1999). Publicly funded research and development (R&D) play an important role in the agricultural sector. Since innovations resulting from public R&D can be considered public goods that firms can adopt in a relatively short period of time, the diffusion process may be faster in agriculture and this may lead to a smaller impact of the business cycle on convergence. These competing arguments underscore the empirical nature of the relationship between the business cycle and convergence and suggest that results obtained for other sectors may not be applicable to the agricultural sector.

Second, the unit of observation is the state. In using aggregate data, we fail to account for the effects of entry and exit of firms from the industry. As a result, our estimates may be biased (see Theil, 1954) and should be interpreted with care. The literature on convergence among states or countries typically ignores exit and entry since it is unlikely that a state or country will exit the market.[5] Nevertheless, if we consider the fact that the farm sector in each state is composed of a finite number of firms, the bias may persist. In particular, the individual firms' decisions may have a non-negligible impact on the behavior of the aggregate variables (see Hart, 1985a,b). If exiting firms are less productive than surviving firms then their exit will contribute to each state's productivity growth, thereby leading to biased results if the entry and exit depend on each state's initial productivity level (Baldwin and Gorecki, 1991; Foster, Haltiwanger and Kriza, 1998; Fujita, 2008). Since we do not observe the initial productivity distribution within the states, determining the magnitude and sign of the bias is outside the scope of this paper.[6]

This paper makes a number of empirical contributions. A common practice in studies of convergence is to include control variables to avoid omitted-variables bias. Most studies include changes in relative capital intensities to capture the effects of technological innovations embodied in capital (see Dollar and Wolff, 1994; Ball, Hallahan, and Nehring, 2004).[7] However, the optimal factor demands depend on TFP growth, so changes in relative capital intensities are not exogenous with respect to changes in productivity (Daveri and Jona-Lasino, 2007). As a result, the improvements achieved by previous studies through the reduction of omitted-variables bias are potentially offset by the introduction of simultaneity bias in their econometric specification.

We address simultaneity bias using an instrumental variables approach. For the growth rate in relative capital intensities, we use several demand-side instruments, including fiscal impulse, monetary shocks, energy prices, the expected growth rates in potential domestic and external demand, and market accessibility to Metropolitan Statistical Areas (MSA). Both the market accessibility and domestic and external demand variables are constructed using the market accessibility function proposed by Harris (1954). Their construction involves geographic and economic data for more than 3,000 counties, 25,000 cities, 300 MSAs, and 80 U.S. ports.

Our data consist of a state-by-year panel. Using asymptotic distributions based on panel data may lead to poor approximations of the actual distributions of the parameter estimates. Therefore, we apply time-series cross-sectional (TSCS) techniques in order to provide reliable standard errors and critical values. We perform unit-root tests for panel data to assess the time-series properties of the data. Then we correct for unobserved heterogeneity at both state- and time-specific levels by considering a two-way error components econometric specification. Finally, we use TSCS Instrumental Variables Feasible GLS regression to obtain parameter estimates that are robust to endogeneity, heteroskedasticity, autocorrelation, and cross-sectional contemporaneous correlation.

The tests of the convergence hypothesis are conditional on a vector of control variables. Following Dollar and Wolff (1994) and Ball, Hallahan and Nehring (2004), we include changes in relative capital intensities to capture technological embodiment.[8] We also include two indicators of agricultural specialization—the relative crop and livestock output intensities—to control for differences in TFP growth rates between the livestock and crop subsectors (see Evenson and Huffman, 2001). And we include years of schooling and worker experience at the state level to capture possible ‘technology spillovers’ from investments in human capital (see Baier et al., 2007: Parman, 2009).[9]

Our empirical results confirm the catch-up hypothesis, showing a highly significant inverse relation between a state’s productivity growth and its initial level of productivity. Technology embodied in capital was an important source of productivity growth. In fact, once we addressed the endogeneity of the relative capital intensities, embodiment was found to be a more important source of productivity growth than previously reported in the literature. Productivity growth was inversely related to specialization. Highly specialized farms were among the productivity leaders, but they exhibited relatively slow rates of productivity growth. There were significant spillovers from investment in human capital, leading to more rapid productivity growth. Finally, although we found evidence of convergence across the different phases of the business cycle, the speed of convergence was greater during periods of contraction in economic activity.

2. Tests of *β*-Convergence

This section presents the econometric models used to test the convergence hypothesis. First, we describe the model gleaned from the literature, termed the benchmark model. Then we present the model used to investigate the relationship between catching-up and the business cycle.

The Benchmark Model

To test the convergence hypothesis, we employ the basic specification:

$$\Delta \ln(TFP_{i,t}) = \alpha + \theta_1 \ln(TFP_{i,t}) + \Theta_x' X_{i,t} + v_{i,t}, \qquad (1)$$

where $TFP_{i,t}$ is state *i*'s productivity level in period *t* relative to the United States average and $X_{i,t}$ is a vector of possibly endogenous control variables.[10] Testing for *β*-convergence is equivalent to testing H_0: $\theta_1 = 0$ (*i.e.*, no *β*-convergence) against H_1: $\theta_1 < 0$ (*i.e.*, *β*-convergence), where $\theta_1 = -(1-e^{\beta})$ and β is the rate of convergence.

The specification in equation (1) implies symmetric mean reversion (SMR). States with TFP levels above the average converge to the mean at the same speed as states with TFP levels below the average. To model asymmetric mean reversion (AMR), we include a dummy variable, $d_{i,t}^{AMR}$, defined as unity if the state's TFP level is above the average, that interacts with $\ln(TFP_{i,t})$:

$$\Delta \ln(TFP_{i,t}) = \alpha + \Theta_1 [D_{i,t}^{AMR} \times \ln(TFP_{i,t})] + \Theta_x' X_{i,t} + v_{i,t}, \tag{2}$$

where

$$D_{i,t}^{AMR} = (1, d_{i,t}^{AMR})',$$

$$\Theta_1' = (\theta_1, \theta_{1,d}),$$

and

$$d_{i,t}^{AMR} = 1[TFP_{i,t} > 1],$$

where $1[\cdot]$ is an indicator function. Testing for asymmetric mean reversion in *β*-convergence is equivalent to testing H$_0$: $\theta_{1,d} = 0$ (*i.e.*, no asymmetric mean reversion) against H$_1$: $\theta_{1,d} \neq 0$ (*i.e.*, asymmetric mean reversion).

Catching-up and the Business Cycle

To characterize the relationship between *β*-convergence and the business cycle, we model changes in the coefficient on the initial level of productivity across the different phases of the business cycle. We also look at the effects of the business cycle on embodiment.

There are two reasons why we would expect asymmetries in the embodiment effect across the business cycle. First, capital and labor reallocations have been shown to have important cyclical patterns (see Eisfeldt and Rampini, 2006; Akerlof, Rose and Yellen, 1988; Foote, 1998). Second, the innovation-imitation hypothesis discussed in the introduction not only suggests that we should observe faster catching-up during periods of contraction, but also stronger embodiment effects. This is because productivity followers tend to learn by imitation, especially in downturns, and the innovations that they imitate may be embodied in capital.

From 1960 to 2004, the U.S. economy experienced seven recessions. Figure 1 shows the year-over-year growth rates of GDP and the National Bureau of Economic Research (NBER) recession dating (boxed area). Two important facts emerge from this figure. First, expansions are longer than recessions (around 6 years on average against 1 year on average). Second, recessions have become less frequent since the middle 1980s. Given this asymmetry, we introduce in equation (2) interaction effects between a set of dummy variables that identify the different phases of the business cycle and the variables of interest, $\ln(TFP_{i,t})$ and $\Delta\ln(K/L)_{i,t}$.

We use the output gap and the NBER's Recession Dating Procedure to identify the different phases of the business cycle. A positive (negative) output gap indicates that the economy is operating below (above) its full employment potential, thereby allowing us to distinguish periods of low economic activity (*i.e.*, contractions and recoveries) from periods of high economic activity (*i.e.*, booms). The NBER's Recession Dating Procedure determines the official peaks and troughs of the business cycle, thus identifying the periods when the economy is officially in a contraction phase (*i.e.*, from a peak to a trough) and, conversely, when in an expansion phase (*i.e.*, from a trough to a peak). Using this information, we partition the business cycle into a contraction phase, say Phase (C), a recovery phase, say Phase (R), and a late expansion phase, say Phase (E).

The model specification used to investigate the relationship between β-convergence and the business cycle is given in equation (3). To simplify the notation, we represent equation (3) assuming there is no asymmetric mean reversion (*i.e.*, that $\theta_{1,d} = 0$):

$$\begin{aligned}
&\Delta \ln(TFP_{i,t}) = \alpha + \Theta'_{(1)}[Phase(BC)_t \times \ln(TFP_{i,t})] \\
&+\Theta'_{(k)}[Phase(BC)_t \times \Delta \ln(K / L_{i,t})] \\
&+\Theta'_{\tilde{x}}\tilde{X}_{i,t} + v_{i,t},
\end{aligned} \tag{3}$$

where

$$Phase(BC)_t = (Phase(C)_t, Phase(R)_t, Phase(E)_t)',$$

$$\Theta'_{(1)} = (\theta_{1,C}, \theta_{1,R}, \theta_{1,E}),$$

$$\Theta'_{(k)} = (\theta_{k,C}, \theta_{k,R}, \theta_{k,E}),$$

and

$Phase(C)_t = 1$ [In period t the U.S. economy is officially in a contraction phase],

$Phase(R)_t = 1$ [In period t the U.S. economy is officially in an expansion phase and the U.S. output gap is positive],

$Phase(E)_t = 1$ [In period t the U.S. economy is officially in an expansion phase and the U.S. output gap is negative],

where $1[\cdot]$ is an indicator function and $\tilde{X}_{i,t}$ is a vector of control variables.

3. Data Description

The production accounts and measures of productivity used to investigate the convergence hypothesis were constructed by the U.S. Department of Agriculture (USDA). The Department's Economic Research Service routinely publishes measures of productivity for the states and the aggregate farm sector defined over states (see Ball et al., 1999; Ball, Hallahan, and Nehring, 2004).[11] The USDA model is based on the translog transformation frontier. It relates the growth rates of multiple outputs to the cost-share weighted growth rates of labor, capital, and intermediate goods.

The applied model is quite detailed. The changing demographic character of the agricultural workforce is used to build a quality-adjusted index of labor input. Similarly, much asset specific detail underlies the measure of capital input. Construction of the measure of capital input begins with estimating the capital stock for each component of capital input. For depreciable assets (i.e., machinery and equipment; structures), the capital stocks are the cumulation of all past investments adjusted for discards of worn-out assets and loss of efficiency of assets over their service life. The capital stocks of land and inventories are measured as implicit quantities derived from balance sheet data. The index of capital input is formed by aggregating over the various capital assets using cost-share weights based on asset-specific rental prices. The contributions of feed and seed, energy, and agricultural chemicals are captured in the index of intermediate inputs. An important innovation is the use of hedonic price indexes in constructing measures of fertilizers and pesticides consumption. Finally, considerable effort was expended to develop output and input indexes that have spatial and temporal integrity. The result is a true panel that can be used for both cross section and time series analysis.

Our tests of the catch-up hypothesis include a number of control variables. We include changes in relative capital intensities, $\Delta \ln(K/L)_{i,t}$, to capture embodiment.[12] We also include indexes of specialization to control for differences in TFP growth rates across agricultural subsectors. To capture possible human capital spillovers, we include differences in years of schooling and worker experience across states. The growth rates of the relative capital intensities and indexes of specialization were constructed using the USDA database, while years of schooling and worker experience were taken from Baier et al. (2007).[13]

Investment in human capital is likely an exogenous source of TFP growth in agriculture, but the growth rates of the relative capital intensities and agricultural specialization may be endogenous. We address potential endogeniety using instrumental variables.

Valid instruments for the capital intensities would be variables that are correlated with the inputs but are orthogonal to TFP shocks. One might conclude that a natural set of instruments would be the lagged values of the endogenous variables (Cungun and Swinnen, 2003). However, these lagged values may not be valid instruments because the optimal input demands may depend on past values of TFP (see Levinson and Petrin, 2000), which leads to a violation of the weak exogeneity conditions. We use two different sets of demand-side instrumental variables. The first set of instruments varies across time periods but not across states, while the second set of instruments varies across both time periods and states.

Following Groth, Nuñez, and Srinivasan (2006), the first set of demand-side instruments includes monetary shocks, proxied by the changes in medium- and long-term interest rates, and fiscal impulse, measured by the changes in the U.S. primary deficit as a percentage of GDP. The second set includes the growth rates in relative energy prices, the expected growth rates in potential domestic and external demand, and market accessibility to Metropolitan Statistical Areas (MSA).[14]

It can be argued that TFP growth also plays a role in determining production patterns (*i.e.*, specialization) across states or regions (see Gopinath and Upadhyaya, 2002), thereby leading to simultaneity bias. We address this problem by considering regional and time fixed effects and by introducing relative chemical and energy input intensities as instruments. The relative chemical and energy intensities are likely highly correlated with our measures of specialization because farms in a particular state that specialize in the production of, say crops, will also have relatively large chemical and energy input shares. In addition, these instruments should be valid sources of exogenous variation (*i.e.*, orthogonal to shocks in TFP) since the intermediate input indexes are adjusted for changes in input quality (see Jorgenson and Griliches, 1967; Ball, Hallahan, and Nehring, 2004).

4. Empirical Results

This section discusses our empirical results. First, we discuss the results of our tests of β-convergence ignoring the business cycle (*i.e.*, the benchmark model). Then we present test results that take into account the effects of the business cycle on the rate of convergence and on embodiment.

The Benchmark Model

Testing for a Panel Unit Root

To minimize the potential for spurious regression results, we first examine whether the variables in equation (2) exhibit a unit root. We perform panel unit root tests proposed by Levin, Lin, and Chu (2002), Im, Pesaran and Shin (2003) and Breitung (2000), respectively. Compared with individual unit root tests, such as the Augmented Dickey Fuller test or the Phillips and Perron (1988) test, all of these have common advantages when dealing with small samples. However, they also have their own limitations, which suggest a joint interpretation of the test results. The Levin, Lin, and Chu (2002) and Im, Pesaran and Shin (2003) tests, for example, face size distortions as the cross-section dimension gets large relative to the time series dimension. On the other hand, the Breitung (2000) and Levin, Lin, and Chu (2002) tests require homogeneity of the first-order autoregressive parameters, which restricts the parameters to be equal across all the cross-sections under the alternative hypothesis (Baltagi, 2005, pp. 235-239). Table 1 summarizes the results of the panel unit root tests. The tests include a constant term and, in the case of TFP growth rates, a time trend. All of the test statistics are less than the critical value of -1.65 at the 5% level. Therefore, we reject the null hypothesis of a unit root and proceed by estimating equation (2) assuming stationarity.

Pooled OLS Estimates

The first column of Table 2 reports pooled OLS estimates of equation (2). The results support the catch-up hypothesis, showing a highly significant inverse relation between the rate of TFP convergence and its initial level. The variable $\Delta \ln(K/L)_{i,t}$ has a positive and significant coefficient, suggesting that embodiment of technology in capital is an important source of TFP growth. The relation between productivity growth and specialization is not statistically significant. Neither is the relationship between productivity growth and years of schooling or worker experience. Finally, the coefficient on the interaction term $d_{i,t}^{AMR} x \ln(TFP_{i,t})$ is not significant, suggesting there is no asymmetric mean reversion. We note, however, that the results in column (1) are consistent if and only if the orthogonality conditions on equation (2) hold (*i.e.*, the explanatory variables are uncorrelated with the error term $v_{i,t}$).

Unobserved State-Specific Effects

To control for unobservable state-specific effects, we perform three tests. First, we perform the Breusch and Pagan (1980) Lagrangian Multiplier test for random effects against the pooled OLS estimates. Then we perform an F-test for fixed effects. Finally, we perform the Hausman (1978) specification test to compare the random- and fixed-effects specifications. The state-specific effects model (or one-way error components model) is given by equation (2) and:

$$v_{i,t} = \eta_i + u_{i,t}, \tag{4}$$

where η_i denotes the unobservable state-specific effect and $u_{i,t}$ is the remainder disturbance. Table 3 shows the results of the tests for state-specific effects. The Breusch and Pagan (1980) test for random effects and the F-test for fixed effects yield *p*-values less than 0.05, which clearly points to the presence of state-specific effects. Furthermore, the Hausman (1978) specification test yields a *p*-value less than 0.01, which confirms that the differences between the random-effects and fixed-effects coefficients are systematic. We conclude that the fixed effects are relevant and that both the pooled OLS and random-effects GLS estimators are inconsistent.

Unobserved Time-Specific Effects

Having confirmed the existence of state-specific fixed effects, we explore the existence of unobserved time-specific effects. For simplicity, we assume that if there exist unobserved time-specific effects common to all the states then these must be fixed effects. This assumption does not compromise the consistency of the estimated parameters. The two-way error components model is given by equation (2) and:

$$v_{i,t} = \eta_i + \varepsilon_t + u_{i,t}, \tag{5}$$

where η_i and ε_t denote the unobservable state- and time-specific fixed effects and $u_{i,t}$ is the remaining stochastic disturbance. To test the time-specific effects hypothesis we estimate the two-way fixed effects model and then perform an F-test for time-specific fixed effects. The null hypothesis is that $\varepsilon_t = 0, t = 1,...,T$.

Column (2) of Table 2 summarizes the two-way fixed-effects estimation results. The F-test for the two-way fixed effects model against the one-way fixed-effects model yields a *p*-value less than 0.01. Therefore, we can reject the null hypothesis at the usual confidence levels. We conclude that both state- and time-specific fixed effects are significant.

Endogeneity of the Regressors

As previously noted, such variables as the relative capital intensities and the indicators of specialization may be viewed as endogenous. We test for endogeneity using the Davidson and MacKinnon (1993) augmented regression test procedure. First, we estimate a two-way fixed effects model for each of the possibly endogenous right-hand side variables in equation (2) using as instruments all the exogenous variables in equation (2) and the excluded instruments described in the previous section. Then we perform the augmented two-way fixed-effects within regressions by including the first-step residuals. If the coefficients on the residuals are significantly different from zero, the original two-way fixed effects estimator is inconsistent (*i.e.*, $E(X_{i,t}, u_{i,t}) \neq 0$

Table 4 reports the endogeneity tests results. The coefficient on the residuals of $\Delta \ln(K/L)_{i,t}$ is significant at the 5% level, indicating that the relative capital intensities are endogenous variables. In the case of specialization, the results are mixed. The coefficient on the residuals of the livestock intensities is significant at the 10% level. But the results suggest that the crop intensities are exogenous since the coefficient on the residuals is not significantly different from zero.

Having determined that a number of the regressors are endogenous, we test the relevance and validity of the instruments with the Kleibergen-Paap (2006) test for underidentification and the Sargan-Hansen (1982) test for overidentifying restrictions. Both tests are robust to heteroskedasticity. The null hypothesis in the underidentification test is that the first-step equations are underidentified (*i.e.*, the excluded instruments are uncorrelated with the endogenous regressors). The joint null hypothesis in the test for overidentifying restrictions is that the instruments are valid (*i.e.*, uncorrelated with the error term $u_{i,t}$) and that the excluded instruments are correctly excluded from the estimation.

Column (3) of Table 2 reports the two-step IV two-way fixed-effects results, while Table 5 shows the results for the underidentification and overidentifying restrictions tests. The Kleibergen-Paap (2006) test for underidentification yields a *p*-value smaller than 0.05, indicating that the excluded instruments are significant. On the other hand, the Sargan-Hansen (1982) test for overidentifying restrictions yields borderline results. The test yields a *p*-value very close to 0.10. Given these results, we conclude that the instruments are valid.

A comparison of the parameter estimates reported in columns (2) and (3) of Table 2 yields two interesting results. First, technology embodied in capital was an important source of productivity growth in the agricultural sector. In fact, once we addressed the endogeneity of the relative capital intensities, embodiment was found to be a more important source of productivity growth than previously reported (see Ball, Hallahan, and Nehring, 2004). Second, productivity growth was inversely related to specialization. Studies by McCunn and Huffman (2000) and Evenson and Huffman (2001) confirm this result. Highly specialized farms were among the productivity leaders, but they exhibited slower rates of productivity growth than did less specialized producers.

Serial Correlation of the Error Components

The specification given by equations (2) and (5) assume that serial correlation in the model stems from the fact that the observations correspond to the same states across the panel. However, the remaining stochastic disturbance $u_{i,t}$ in (5) may be serially correlated. In general, if the autocorrelation problem is not corrected, the Gauss-Markov assumptions about the residuals will be violated and this will lead to consistent but inefficient parameter estimates, as well as biased standard errors (see Baltagi, 2005, p. 84). The generalized two-way fixed effects model with AR(1) remainder disturbances is given by equations (2), (5), and

$$u_{it} = \rho u_{it-1} + e_{i,t}; |\rho| < 1, \tag{6}$$

where $e_{i,t}$ denotes the remaining stochastic error.

Column (4) of Table 2 reports the parameter estimates for the two-way fixed-effects specification with AR(1) remaining disturbances. The results were obtained using the two-step IV method. First, we estimate the endogenous right-hand side variables in equation (2) using a two-way fixed effects model and the set of valid instruments described above. Then we estimate the two-way fixed effects model with AR(1) disturbances using the fitted values of the first-step dependent variables as exogenous variables. Table 6 reports the AR (1) estimated coefficient $\hat{\rho}$, as well as the Baltagi and Li (1995) and Wooldridge (2002) test statistics for the non-serial correlation hypothesis.[15] Both tests yield *p*-values less than 0.01, hence we can reject the null hypothesis of no serial correlation. Given that some of the explanatory variables in equation (2) are endogenous, this result confirms that lagged values of these explanatory variables may not be used as instruments since this would violate the weak exogeneity conditions.

Heteroskedasticity

In order to control for possible groupwise heteroskedasticity, we perform the Modified Wald test in the specification given by equations (2) and (5). Note that this test gives valid results even though the normality assumptions do not hold (see Green, 2003). They are also robust to endogeneity. First, we estimate the endogenous right-hand side variables in equation (2) using a two-way fixed effects model and the above set of valid instruments. Then we estimate the two-way fixed-effects model using as instruments the fitted values for the first-step dependent variables. Finally, we perform the Modified Wald test. The test yields a *p*-value less than 0.01. Thus we can reject the null hypothesis of homoskedasticity.

Final Benchmark Specification

The final benchmark specification (*i.e.*, before introducing the effects of the business cycle) is a two-way fixed effects model with state-specific error variances and state-specific AR(1) disturbances:

$$v_{it} = \eta_i + \varepsilon_t + u_{i,t}, \tag{7}$$

$$u_{it} = \rho_i u_{it-1} + e_{it}; |\rho| < 1. \qquad (8)$$

In order to correct for endogeneity, heteroskedasticity and autocorrelation, we proceed by estimating the model using TSCS Instrumental Variables Feasible GLS regression. First, we estimate the endogenous right-hand side variables in equation (2) using a two-way fixed effects model and the set of valid instruments described above. Then, using the fitted values for the first-step dependent variables, we estimate using TSCS Feasible GLS the two-way fixed-effects model robust to endogeneity, hetersoskedasticity, autocorrelation and cross-sectional contemporaneous correlation. We include dummy variables for each year and each state to control for state- and time-specific fixed effects.

The empirical results for our benchmark model are summarized in column (5) of Table 2. Note that the results in column (5) confirm that human capital spillovers contribute significantly to TFP growth. Moreover, there is evidence of asymmetric mean reversion. Those states with below average TFP levels converge to the mean level at a faster rate than states with TFP levels above the average.

Impact of the Business Cycle

We investigate the impact of the business cycle on convergence using the specification in (3) above. This specification captures the effects of the business cycle through interactions with the initial level of productivity. We also include interaction terms with the relative capital intensities to capture the effects of the business cycle on embodiment.

Table 7 reports the parameter estimates. The results confirm a tendency toward convergence in levels of TFP across the different phases of the business cycle. Table 8 presents the Wald χ^2-tests for differences in the rates of convergence across the different phases of the business cycle. The χ^2-test for differences in the rates of convergence during the contraction and early recovery phases of the business cycle yields a *p*-value greater than 0.10. However, the test for differences in the rates of convergence during the contraction and late expansion phases yields a *p*-value less than 0.05. The Wald χ^2-tests for differences in the embodiment effect during the contraction phase and the recovery and late expansion phases of the business cycle yield *p*-values less than 0.01 We conclude that there is a small but statistically significant difference in the rates of convergence during the contraction and late expansion phases of the business cycle, while there are large and statistically significant differences in the embodiment effect during the contraction phase and the recovery and late expansion phases of the business cycle.

The specification in (3) also allows for asymmetric mean reversion. When we allow for asymmetric mean reversion, the rate of convergence for productivity followers is 4.8% faster during the contraction phase than during the late expansion phase. The difference is even greater for the productivity leaders, about 5.1%. Finally, the embodiment effects are 33.9% and 73.7% greater during the contraction phase of the business cycle than during the recovery and late expansion phases.

5. Concluding Remarks

This paper examines the relation between the business cycle and convergence in levels of agricultural productivity across the U.S. states. First, we test the catch-up hypothesis using a model specification that ignores cyclical fluctuations in economic activity (*i.e.*, our benchmark model). Then we show how the rates of convergence change across the different phases of the business cycle. We also assess the impact of the business cycle on embodiment.

The results from our benchmark model can be summarized as follows. First, we found evidence of convergence in levels of productivity. Second, embodiment was found to be an important source of TFP growth in agriculture. In fact, after correcting for endogeneity of the relative capital intensities, embodiment was found to be a more important source of productivity growth than was previously reported in the literature. Third, productivity growth was inversely related to specialization. Highly specialized farms were among the productivity leaders but they exhibited slower rates of productivity growth than did less specialized producers. We conclude that increased specialization is a way to affect productivity gains. Finally, we found that there are important human capital spillovers in agriculture. States with higher levels of educational attainment and worker experience achieved faster rates of productivity growth.

While there is evidence of convergence across the different phases of the business cycle, the speed of convergence is greatest during periods of contraction in economic activity. After allowing for asymmetric mean reversion, we found that the convergence rate for productivity followers was 4.8% higher during contractions than during late expansions. These differences were even greater for the productivity leaders, about 5.1%.

We also found significant differences in the magnitude of the embodiment effect across the different phases of the business cycle. The embodiment effect was 33.9% and 73.7% larger during contractions than during recoveries and late expansions.

Finally, the expected pattern of convergence across the business cycle finds some empirical support. This pattern is the result of the pro-cyclical nature of the innovation process and the time lags in the diffusion of technical information. In contrast with evidence from the manufacturing sector, however, the magnitude of the effects of the business cycle through the rate of convergence appears to be smaller in the agricultural sector. We attribute this to public funding of R&D in the agricultural sector. Since innovations resulting from public R&D can be considered public goods that firms can adopt relatively quickly, the diffusion of technical information will be more rapid in agriculture and this points to a smaller impact of the business cycle on TFP convergence. There are also implications for the design of policy. Public investment in agricultural research can be an effective tool in addressing regional disparities in income.

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Notes

1. Ball et al. (2001), in a study of agriculture in twelve OECD countries, found evidence of convergence in levels of productivity. Moreover, the dispersion of their relative levels (as measured by the coefficient of variation) decreased over time.

2. Their tests for convergence are conditional on these variables. In the growth literature, this is referred to as conditional convergence. However, for simplicity of exposition we use the term convergence.

3. Dumagan and Ball (2009) provide a decomposition of changes in revenue into its components. This decomposition reveals that productivity growth in agriculture accounted for nearly two-thirds of the growth in revenue over the postwar period. The authors conclude that policy should focus more on measures to foster productivity growth (e.g., public funding of research) than often adopted price support programs to enhance growth in income.

4. Overshooting of prices refers to temporary changes beyond long-run equilibrium levels.

5. A particular case in which this can happen is when we only consider export markets where we observe zero trade flows between specific pairs of countries (see Helpman, Melitz, and Rubinstein, 2008).

6. If most exiting farms were concentrated in states with lower initial aggregate productivity the bias would be negative (*i.e.*, biased towards β-convergence). If most exiting farms were concentrated in the states with higher initial aggregate productivity, the bias would be positive (*i.e.*, biased against β-convergence). Finally, if there were no statistically significant differences in the exit rates between the most productive states and the less productive states the results would be unbiased.

7. Ball, Hallahan, and Nehring (2004) also allowed for embodiment of technology in materials inputs, but the estimated effect was not statistically significant. They attributed this result to adjusting the input indexes for quality change.

8. The capital intensities are defined over machinery and equipment and non-residential structures.

9. We use the term 'spillovers' because our measures of educational attainment and worker experience pertain to the total workforce in each state as opposed to the

agricultural workforce in that state.

10. In the most basic specification, only the initial and final periods are considered. We, on the other hand, construct growth rates for overlapping periods. The advantage of using overlapping periods is that the estimates are less sensitive to starting and ending dates.

11. The production accounts are available electronically at: http://www.ers.usda.gov/data-products/agricultural-productivity-in-the-us.aspx

12. It is likely that at least some technological innovation is embodied in materials inputs such as fertilizers and pesticides. However, the input quantities are measured implicitly using hedonic price indexes; they are adjusted for changes in input quality. The resulting input measures will be uncorrelated with changes in productivity (see Jorgenson and Griliches, 1967; Ball, Hallahan, and Nehring, 2004).

13. Baier et al. (2007) use a perpetual inventory method to construct average years of schooling and experience of the work force for each state. The data span the years 1840 to 2000. Estimates for the years 2001-2004 were extrapolated using TRAMO. TRAMO is a program for MLE of regression models with general non-stationary errors, outliers, and long sequences of missing observations (see Gómez and Maravall, 1997; Maravall, 2005).

14. A complete description of methods and data used to construct the market accessibility and domestic and external demand variables is provided in an appendix available from the authors.

15. We perform the Baltagi and Li (1995) test and Wooldridge (2002) test since both tests can be applied under very few maintained assumptions.

Figure 1: U.S. GDP and NBER-Dated Recession.
Source: NBER.

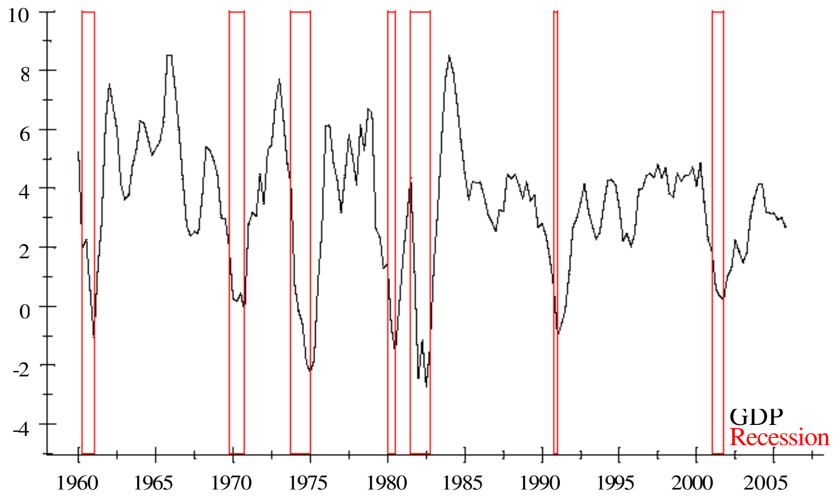

Table 1: Panel Data Unit Root Tests

Variable	LLC Statistic	IPS Statistic	BRG Statistic
Δ ln $(\text{TFP})_{I,t}$	—44.905	—50.343	—24.285
ln $(\text{TFP})_{I,t}$	—18.125	—16.027	—9.881
$\Delta \ln(K/L)_{i,t}$	—47.091	—46.115	—34.825
$\text{Livestock}_{i,t}$	—17.152	—15.987	—8.101
$\text{Crops}_{i,t}$	—17.726	—17.240	—7.162
Δ $\ln(\text{Schooling})_{I,t}$	—32.788	—31.572	—19.755
Δ $\ln(\text{Experience})_{I,t}$	—23.955	—20.487	—23.311

Cross-sections included 48
Total panel (balanced) observations: 2112

Notes: Asymptotically standard normal distributed test statistics, 5% critical value —1.65. Automatic selection of lags based on SIC criteria. Newey-West bandwidth selection using Bartlett kernel.

Table 2: Benchmark Model

Dependent Variable: $\Delta \ln TFP_{i,t}$

Method: (1) Pooled OLS; (2) and (3) IV-FE (within regression); (4) IV-FE (within regression); (5) IV-FGLS

Variable	(1)	(2)	(3)	(4)	(5)
$\ln TFP_{i,t}$	-0.0719	-0.3740	-0.3000	-0.5782	-0.2995
	[0.012]***	[0.023]***	[0.061]***	[0.041]***	[0.012]***
$d_{i,t}^{AMR}$ x $\ln TFP_{i,t}$	0.0320	-0.0257	-0.0074	0.0425	0.0253
	[0.024]	[0.042]	[0.076]	[0.049]	[0.010]***
$\Delta \ln(K / L)_{i,t}$	0.2092	0.1756	0.8345	1.0536	0.8624
	[0.014]***	[0.013]***	[0.233]***	[0.235]***	[0.043]***
$Livestock_{i,t}$	-0.0063	-0.0205	-0.3145	-0.5340	-0.4570
	[0.136]	[0.136]	[0.123]***	[0.136]***	[0.028]***
$Crops_{i,t}$	-0.0084	-0.0946	-0.4445	-0.7417	-0.5946
	[0.014]	[0.023]***	[0.138]***	[0.153]***	[0.031]***
$\Delta \ln(Schooling)_{i,t}$	-0.2886	-0.3985	-0.2388	-0.0088	0.1942
	[0.312]	[0.334]	[0.564]	[0.364]	[0.063]***
$\Delta \ln(Experience)_{i,t}$	0.0842	-0.0365	-0.2005	0.2216	0.2091
	[0.144]	[0.160]	[0.248]	[0.187]	[0.030]***
Constant	-0.0130	-0.0508	-	-0.1136	-0.2147
	[0.003]***	[0.010]***		[0.018]***	[0.011]***
Cross-sections included:	48	48	48	48	48
Total panel (balanced) observations:	2112	2112	2112	2112	2112

Notes: * Significant at 10%; ** Significant at 5%; *** Significant at 1%.Standard errors in brackets. All regressors use state and year fixed effects and are robust to autocorrelation. The results are corrected for endogeneity. Instrumented variables: $\ln(K/L)_{I,t}$, $Livestock_{i,t}$.

Table 3: Panel Data State-Specific Effects Tests

Cross-section random effects	
BPLM X^2-statistic	4.99
p-value	0.0255
Cross-section fixed effects	
F-statistic	10.15
p-value	0.0000
Cross-section fixed effects vs Cross-section random effects	
Hausman X^2-statistic	489.06
p-value	0.0000

Cross sections included: 48

Total panel (balanced) observations 2112

Table 4: Panel Data Endogeneity Tests

Variable	
$\Delta \ln(K / L)_{i,t}$	-0.4481
	[0.180]**
$Livestock_{i,t}$	0.2254
	[0.125]*
$Crops_{i,t}$	0.1190
	[0.184]

Cross sections included: 48
Total panel (balanced) observations 2112

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors in brackets.

Table 5: Identification Tests

IV Identification tests (Instrumented: Δln(K/L)i,t, Livestocki,t)	
Underidentification test	
χ^2-statistics	15.350
p-value	0.0318
Overidentification of all instruments	
χ^2-statistics	10.683
p-value	0.0987

Cross sections included: 48
Total panel (balanced) observations 2112

Table 6: Serial Correlation Tests

AR(1) Reminder Disturbances Tests	
$\hat{\rho}$	-0.2608
BLI χ^2-statistic	37.944
p-value	0.0000
WD F-statistic	297.079
p-value	0.0000

Table 7: Catching-Up and the Business Cycle

Dependent Variable: $\Delta \ln TFP_{i,t}$

Method: IV-FGLS

Variable	
Phase(C)$_t$ x ln(TFP)$_{i,t}$	-0.3103 [0.013]***
Phase(R)$_t$ x ln(TFP)$_{i,t}$	-0.3070 [0.012]***
Phase(E)$_t$ x ln(TFP)$_{i,t}$	-0.2985 [0.012]***
$d_{i,t}^{AMR} x \ln TFP_{i,t}$	0.0251 [0.010]***
Phase(C)$_t$ x $\Delta \ln(K/L)_{i,t}$	1.1343 [0.052]***
Phase(R)$_t$ x $\Delta \ln(K/L)_{i,t}$	0.8473 [0.044]***
Phase(E)$_t$ x $\Delta \ln(K/L)_{i,t}$	0.6529 [0.047]***
$Livestock_{i,t}$	-0.4390 [0.028]***
$Crops_{i,t}$	-0.5715 [0.030]***
$\Delta \ln(Schooling)_{i,t}$	0.2136 [0.062]***
$\Delta \ln(Experience)_{i,t}$	0.2059 [0.030]***
Constant	-0.2079 [0.011]***

Cross-sections included: 48

Total panel (balanced) observations: 2112

Wald χ^2 -statistic: 9.8e+06

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors inbrackets. All regressions use state and year fixed effects and are robust to autocorrelationheteroskedasticity and cross-sectional contemporaneous correlation. The results are corrected for endogeneity. Instrumented variables: ln(K/L)$_{I,t}$, Livestock$_{i,t}$.

Table 8: Wald χ^2–test Results for Differences in Rates of Convergence and Embodiment

Differences in β-convergence rates	
H_0 : $Phase(C)_t \times \ln(TFP)_{i,t} - Phase(R)_t \times \ln(TFP)_{I,t} = 0$	
χ^2-statistic	0.33
p-value	0.5672
H_0 : $Phase(C)_t \times \ln(TFP)_{i,t} - Phase(E)_t \times \ln(TFP)_{i,t} = 0$	
χ^2-statistic	4.21
p-value	0.0402
Differences in embodiment effects	
H_0 : $Phase(C)_t \times \ln(K/L)_{i,t} - Phase(R)_t \times \ln(K/L)_{i,t} = 0$	
χ^2-statistic	37.20
p-value	0.0000
H_0 : $Phase(C)_t \times \ln(K/L)_{i,t} - Phase(E)_t \times \ln(K/L)_{i,t} = 0$	
χ^2-statistic	97.73
p-value	0.0000